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pp. 287-298

Economic Growth and the Problem of Inflation[1]

By Nicholas Kaldor

Part II

We ended the last lecture by suggesting that the rate of return on capital in a growing economy will be a function of the rate of growth of incomes, and of the propensities to save out of profits and out of non-profit incomes. If we neglect non-profit savings (consisting of both personal savings and government savings) on the ground that they are largely balanced by non-business loan expenditure, the rate of return reduces to the simple formula: the rate of growth of profits divided by the proportion of profits saved. (In a steadily growing economy, however, distributive shares will be constant over time, and the rate of growth of profits will be the same as the rate of growth of the national income.)

The Supply Price of Risk Capital

This formula focuses attention on the factors which govern the rate of profit, actual or expected, on capital invested in business ventures—it thus governs the demand function for investment or, as Keynes put it, the marginal efficiency of capital. In order that investment should take place, it is necessary that this rate of return should be equal to, or higher than, the supply price of risk capital. According to Ricardo, capital accumulation will only continue when the rate of profit is high enough " to afford [the farmers and manufacturers] an adequate compensation for their trouble, and the risk which they must necessarily encounter in employing their capital productively ".[2]

This particular statement of Ricardo's is really an early statement—perhaps the earliest statement—of what has since come to be known as the " liquidity preference theory ". For let us note that the factors " risk and trouble " set a minimum price, not to the supply of savings, but merely to the productive employment of wealth as against other forms of holding wealth which do *not* involve " risk and trouble ". These are essentially similar therefore to the illiquidity risk on which Keynes concentrated attention, and which causes the yield of long-term bonds to stand higher than the *normal* (or expected) level of the short-term rate of interest. Indeed, by focusing attention on the liquidity premium which the holding of *money* commands as against the holding of gilt-edged bonds, Keynes inadvertently diverted attention from the fact that gilt-edged securities themselves possess considerable advantages from the point of view of liquidity, both in relation to less marketable securities and even more in relation to investment in real assets such as factories or houses. The additional yield of, say, house property in relation to gilt-edged is a reflection, not mainly of the uncertainty concerning the future level of rents, but of the easy marketability of gilt-edged in relation to house property, which makes it possible for the investor to consider gilt-edged holdings as a form of reserve that can be readily " switched " into other forms as and when profitable investment opportunities present themselves; this easy marketability is certainly absent in investment in real property (or plant and equipment). Moreover, the premium which gilt-edged securities command as against direct investment in fixed assets may be quantitatively far more important than the liquidity premium which money and short-term paper commands in relation to gilt-edged securities. According to the calculations of Professor Phelps Brown, the rate of return on capital in Britain in the second half of the nineteenth century (in 1870–1914) was steady around the level of 10 per cent. (on industrial capital alone it was 15 per cent.) at the time when the yield of Consols moved around 3–3½ per cent.[1] On this calculation the premium on investments in financial assets of prime security as against the " productive employment of wealth " amounted to at least 6–7 per cent. No doubt this compensation covers other elements beside the illiquidity risk in a narrow sense—all of which were summed up by Ricardo under " risk and trouble ". But the illiquidity risk—that is, the risk of not being able to withdraw from a commitment, once entered into, should the owner wish to change the disposition of his assets subsequently—is undoubtedly an important element in itself, as is indicated by the fact that businesses do not normally expect the same kind of return on investments in working capital (which are regarded as part of the " liquid assets " of the business) as on investments in fixed assets.

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[1] This article contains the substance of the second of two public lectures delivered at the London School of Economics and Political Science on February 6 and 13, 1959. The first lecture was published in the August issue of this journal.

[2] *Principles*, in Ricardo, *Works*, Sraffa ed., p. 122.

The necessary margin by which the expected return on any particular investment project must exceed the " pure " long-term rate of interest in order to qualify for adoption is influenced also by the level of taxation of profits in the form of income tax as well as profits tax, though more so by the latter than the former. Though nobody knows what this minimum margin is at the present time, at a guess I would put it at 10 per cent gross or 5 per cent net of taxation, which would make the necessary minimum rate of return come to 15 per cent when the pure long-term rate of interest is around 5 per cent.

The fact that the rate of interest cannot be reduced to zero, or rather to below some minimum which is necessarily higher than zero, sets a certain minimum limit to the supply price of risk capital, which is in itself fairly high. Thus if the differential is assumed to be 10 per cent. and the minimum pure long-term rate of interest, below which it cannot be brought by any amount of monetary liquidity, is 2 per cent., the minimum rate of profit necessary to secure the inducement for continued investment is 12 per cent. This in turn is only consistent with the continued accumulation of capital when the rate of growth of income exceeds a certain minimum—a minimum which cannot be calculated precisely (since the savings parameters in our equation can only be guessed within limits) but which is likely to be between one-half and one-third of this rate, that is to say, between 4 and 6 per cent. per annum.

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[1] Cf. E. H. Phelps Brown and B. Weber, " Accumulation, Productivity and Distribution in the British Economy, 1870–1938 ", *Economic Journal*, June 1953, p. 286.

Money Growth and Real Growth

It is here, in my opinion, that the question of rising or falling prices assumes critical importance. For the determinant of the *money* rate of return is the rate of growth of income in *money* terms, which will exceed or fall short of the *real* rate of growth according as prices are rising or falling. If this is correct, a regime of completely *stable* prices is only consistent with a steadily growing economy when the real rate of growth in the national income is fairly high—when it exceeds 4–6 per cent. per annum. *A fortiori*, a policy of stable incomes and falling prices—which so many economists, starting from the Swedish economist Davidson at the turn of the century, have regarded as a kind of ideal—is not consistent with growth at all: not unless the population increased so rapidly that *aggregate* incomes grew at the required minimum rate, despite the fact that incomes *per head* were constant.

Thus in this country the gross national product in money terms grew since 1946 at a compound rate of 7½ per cent. per annum, while the real rate of growth was just under 3 per cent. The annual rise in output prices was thus around 4½ per cent. and the pure long-term rate of interest rose gradually from a low of 2½ per cent to around 5 per cent. Taking the average rate of profit at 2¾ times the rate of growth, the *money* rate of profit comes to 20 per cent. But the *real* rate of profit on this assumption comes to only just above 8 per cent., which would not have sufficed, on our hypothesis of a 10 per cent. minimum margin to give adequate inducements, unless the rate of interest was *minus* 2 per cent. This is precisely what the inflation had done; it reduced the *real* rate of interest to negative levels throughout most of the period, and to around zero during the recent period of relatively high interest rates.

It follows that price stability is only consistent with steady growth when the rate of growth of productivity and/or of the working population is sufficiently large to give a relatively high rate of growth to the total national product. In a weakly growing economy, price stability will mean stagnation unless the propensity to consume is raised sufficiently to offset the effect of a lower rate of growth of profits through a higher share of profits in total income. Thus if the savings propensities were halved, the share of profit in income would be doubled at any given ratio of investment to output, and the rate of profit on capital would be doubled at any given rate of growth of income. (But the requirement of combining a régime of stable prices with a high and growing share of profits in the national income makes the policy objective of avoiding any excessive increase in money wages the more Utopian.)

If the rate of profit is insufficient for *steady* growth, this does not mean that the system will relapse into permanent stagnation—if it did, the past history of capitalist economies could not have exhibited the trend rate of growth which it has shown. But what it does mean is that the process of accumulation and growth is periodically interrupted: periods of accumulation tend to get telescoped into a certain proportion of the years; progress proceeds by fits and starts, and not at a steady rate. Given a sufficiently high rate of growth of productivity (and/or of the working population), or given an adequate supplement to the real rate of growth in the guise of inflation, there is no reason why " booms " should not be perpetual. There is no reason, in other words, to regard the trade cycle as inevitable, provided that money incomes can be kept rising at a rate that is both adequate and steady.

Hence a slow and steady rate of inflation provides a most powerful aid to the attainment of a steady rate of economic progress. One of the few economists of our generation who seems clearly to have perceived this point, in his early writings, is Professor Sir Dennis Robertson who originated the idea, and who coined the phrase, of the policy of the " gently rising price-level ". He was well aware, as shown by his book, *Money*, that a progressive rise in the price-level " so long as it is not so blatant as to generate social disorder or sap the foundations of contract . . . stimulates the production of goods : by benefiting the pockets of the controllers of industry stimulates also their energies and activities: . . . and this fillip to production, by adding to the flow of goods, serves to moderate the very rise in prices which gives it birth ". Hence " so long as the control of production is in the hands of a minority, rewarded by means of a fluctuating profit, it is not impossible that a gently rising price-level will in fact produce the best attainable results, not only for them but for the community as a whole. And it is tolerably certain that a price-level continually falling, even for the best of reasons, would prove deficient in those stimuli upon which modern society, whether wisely or not, has hitherto chiefly relied for keeping its members in full employment and getting its work done ".[1] On the other hand, Keynes, who had such a remarkable intuition in this field, was curiously blind to the implications of continually rising money wages for the inducement to invest. He believed that with the progressive accumulation of capital there would be a gradual slowing-down in economic progress leading to a fall in the marginal efficiency of capital which in turn would lead to an ultimate Day of Judgment (as Professor Pigou called it) when the rate of interest fell to the absolute minimum governed by liquidity preference and when further accumulation and progress necessarily came to an end. He certainly failed to perceive that the simple expedient of allowing money wages to rise faster than productivity—which to us, these days, seems such an easy thing to do—is enough to lay this particular ghost indefinitely. If the *money* rate of interest cannot be brought down below a certain floor, the *real* rate of interest certainly can *crash* through it—indeed, it may be difficult to prevent it from falling too far ! Granted the fact that in the last resort we can always have recourse to a little inflation there is really no reason why an unfavourable constellation between Liquidity Preference and the Marginal Efficiency of Capital should bring capitalism to its ultimate doom.

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[1] *Money*, 1922, pp. 122–125.

Having said all this, I do not wish to leave you with the impression that the inflation of the post-war era was the best in the best of all possible worlds. While it is highly probable that some inflation in the circumstances was necessary to provide adequate stimuli to continued growth, the actual extent of the inflation does appear to have been greater than that required for the purpose. This is shown by the fact that rising investment levels (both absolutely and as a proportion of the national income) were combined with a fairly sharply rising trend in the money rates of interest since 1948—a clear indication that the money rate of profit exceeded the rate of interest by more than the required minimum.

As I mentioned before, the actual rate of growth of money incomes, both wages and profits, was around 7½ per cent. a year. It seems probable, though one cannot of course be certain, that much the same levels of employment, real investment and rates of productivity growth would have been attained if the rate of increase of money wages and money profits had been only around 4–5 per cent. a year. On the other hand I would be worried about a policy which restricted the rate of increase in money wages to the 2–2½ per cent. which corresponded to the rate of growth of real output per man: not that the prospects of attaining any such objective are sufficiently great to give one much cause for anxiety. (In the majority of industrialised countries in the post-war era, and not only in Britain, annual wage increases averaged 5–8 per cent. a year.) But a target for annual wage increases of 4–5 per cent. a year does not seem to me beyond the bounds of possibility; nor does it seem to me beyond reach so to improve the technical dynamism of the British economy as to raise the rate of growth of productivity to somewhere near that figure. In other words, the objective of price stability could be attained, and could only be attained consistently with economic growth, by a combination of measures that would on the one hand reduce the rate of wage inflation, and on the other hand raise the rate of growth of productivity so that the two rates would ultimately converge in the middle, so to speak.

In the remaining part of this lecture I should like to go in more detail into these two critical aspects of the problem—the rate of increase in money wages and the rate of growth of labour productivity.

Causes of Wage Increases

Amid the welter of conflicting views on inflation it is reassuring to find a fair unanimity among economists on the key role of the rate of increase in money wages in the inflationary process. Without a continued rise in money wages inflation could not go on as a *process* in time—since whatever forces were present in the economy making for a rise in prices, they could only have caused a once-and-for-all rise in prices which would in itself have served to eliminate the excess demand that gave rise to it. It is the rise in wages which governs the *increase* in monetary demand, and thus the rate of increase in incomes in general. It is generally agreed also that the rate of increase in money wages has little if any influence on the *share* of wages in the national product: when money wages rise at a faster rate, money profits will also rise at a faster rate; there is no reason to suppose that wages over any period of time stand higher in relation to profits over that period than they would have stood with a lower rate of increase in money wages.

It is when we come to analyse the factors which determine the increase in money wages that there is a conflict of opinion between those who believe that it is governed by the pull of excess demand in the labour market—by the competitive bidding of employers for labour, each offering jobs a little above the prevailing level of rates in order to attract labour from other employers—and those who believe that it is mainly the outcome of collective wage negotiations and reflects the pressure for wage increases from the side of the unions. On the former view the role of collective wage negotiations is mainly to put an official stamp, so to speak, to the wage increases that would have come about in any case, under the pressure of market forces; while on the latter view, competitive bidding for labour by employers accounts for a small part of the annual rise in wages, represented by *some* of the excess in the rise in actual earnings over the increase in negotiated levels. (In Britain this excess averaged 1 per cent a year since 1948, while the rise in negotiated wages averaged 6 per cent. annually. Assuming that one half of this excess was due to the wage-drift caused by competitive bidding, the other half reflecting the effect of overtime and piece-rates, the " demand-pull " element, on this latter view, accounted for only about one-twelfth of the rise in wages that had taken place.)

The demand-pull theory assumes a degree of perfection in the labour market which is unrealistic. It relies on the assumption that when production is limited by labour shortages it pays an individual employer to offer higher wages if thereby he can attract labour from other employers and thus increase his own output. But whether it does so or not depends on whether it is possible for him to offer higher rates to newcomers without increasing at the same time the wages of his existing workers; and on the number of additional men the employer wishes to engage in relation to his existing labour force. A large establishment clearly could not discriminate in favour of new employees without a serious deterioration of labour relations; added to which is the fact that the elasticity of supply of labour to a large employer may not be very large. Hence it is only in cases where the individual employer hires only a single employee or a few employees (as, for example, in domestic service) that a shortage of labour is likely to exert an upward pressure on the wage level from the side of demand.

Recently Professor Phillips has published some interesting calculations[1] showing that there was a strong negative correlation in the 1861–1913 period (and to some extent also in the post World War II period) between the rate of increase in money wages on the one hand and the level of unemployment *and* the rate of change of unemployment on the other hand. I think he has established the existence of these relationships; but I do not believe that they support the particular inference which he draws from them (even for the pre–1913 period) —i.e., the inference that the rise in wages reflects mainly the competitive bidding for labour by employers, with " employers bidding more vigorously for the services of labour in periods of *increasing* demand than in a year during which the average percentage of unemployment was the same but the demand for labour was not increasing "[2] I think his figures are perfectly compatible with the alternative theory —indeed, I am prepared to argue that they provide a better support for that theory than for his own.

On this alternative theory the rise in money wages depends on the *bargaining strength* of labour; and bargaining strength, in turn, is closely related to the prosperity of industry, which determines both the eagerness of labour unions to demand higher wages and the willingness and ability of employers to grant them. It is when investment is high that profits are high, and it is in periods of rising total production and rising productivity that profits are rising. Such periods in turn are periods of low unemployment, and also periods of falling unemployment. If instead of relating wage increases to unemployment and the rate of change of unemployment, Professor Phillips had related them to the increase in production, or to the increase in profits of the previous year, I am confident that he would have found an even better correlation—for *all* his periods, inter-war and post-war, as well as pre-war, excepting perhaps those years when Sir Stafford Cripps was Chancellor and a policy of voluntary restraint by the labour unions was in operation.

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[1] A. W. Phillips, " The Relation Between Unemployment and the Rate of Change of Money Wage Rates in the United Kingdom, 1861–1957 ", *Economica*, November 1958.

[2] *Ibid.*, p. 283.

Prosperity, and rising prosperity, determine the bargaining strength of labour, simply because in most cases—with the exception perhaps of some large monopolies or oligopolies—neither employers nor unions take into account the rise in prices and profits that would *result* from wage increases, so that the scope for wage increases is limited by what can be granted out of the profits earned at the *existing* level of prices. No doubt wage increases lead to a rise in prices as well as costs, but in most cases this process is an indirect one, operating through the subsequent rise in the level of demand; it is not something that is taken for granted, and brought into the calculation beforehand. (If this were not so, there is no reason why employers should resist demands for increased wages—indeed, they ought to welcome them.) In practice, the general rate of increase in the wage level is largely determined by the rate of increase in wages in leading industries, and it is the *already attained* increase in profits of these industries, whether resulting from a previous increase in demand or from a rise in productivity, which governs the increase in negotiated wages. Added to this is the fact that the employers' eagerness to come to a settlement is closely related to the prevailing state of business activity. In times of low order books, the prospect of a strike, involving a shut-down of the works for a certain period, does not frighten the employers; in certain circumstances they might even welcome it if it enables them to lower costs by telescoping operations into a shorter period. It is in times of high business activity that strike action is both costly and embarrassing to the employers. It is these factors, and not the competition of unemployed labour as such, which often make the bargaining strength of labour weak in times of relatively high unemployment.

There is thus a complicated interaction, in a growing economy, between the rise in profits and the rise in wages. The rise in production leads to a rise in profits as such; this in turn leads to a rise in wages which, by increasing demand further, causes a faster rate of increase in profits. It is therefore more correct to speak of a profit-wage spiral than of a price-wage spiral: for the rise in wages is prompted by the rise in profits, irrespective of whether the rise in profits was accompanied by any (or by a corresponding) rise in prices. As Professor Phillips concludes, there is no evidence that the rise in the cost of living is normally a significant factor in determining the rate of increase in wages. The cost-pull or wage-pull is not motivated by the rise in the cost of living: it is motivated by the rise in profits which governs the employers' ability to grant wage increases; and though this process is consistent with a shortage of labour for its operation, it does not by any means presuppose such a shortage.[1]

As against this, the demand-pull theory favoured by Professor Phillips is up against a host of difficulties. For one thing, it cannot explain why, in the Crippsian period, 1949–1950, the rise in both wages and earnings should have been so moderate (the rise in wages averaged less than 3 per cent. in these two years) when unemployment was no greater than in the surrounding years. If the rise in wages is primarily motivated by the competitive bidding for labour by employers, an appeal for voluntary wage restraint to the unions would merely increase the gap between the rise in negotiated wages and the rise in earnings; it would not slow down the latter. Secondly, the demand-pull theory cannot explain why periods of rising production and profits should cause a rapid rise in wages, even in times of high unemployment—as for example, in 1936 and 1937, when wages rose at comparable rates to the post-war years, despite the fact that unemployment averaged 11–13 per cent of insured workers. Finally, the demand-pull theory can hardly explain why the rate of wage inflation in countries like Germany should have exceeded that of Britain, despite the fact that, owing to the rapid rise in the working population, a period of exceptional production growth was associated with the persistence of considerable unemployment.

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[1] This is best shown in not-fully-developed economies like Italy, where wages in the industrial sector habitually rise at least as fast as in the " full-employment " economies, despite a great superfluity of labour.

Price Stability and Unemployment

I have dwelt on Professor Phillips' views at some length simply because a misguided diagnosis of the causes of wage inflations can play such a vital role in policy decisions which are of the utmost importance to the economy. Professor Phillips' conclusion from his investigation is, that, assuming an increase in productivity of 2 per cent. a year, " if aggregate demand was kept at a value which would maintain a stable level of product prices, the associated level of unemployment would be a little under 2½ per cent. If, as is sometimes recommended, demand were kept at a value which would maintain stable wage rates, the associated level of unemployment would be about 5½ per cent.[1] The clear implication of this is that it is possible through the regulation of effective demand to combine steady growth with stable prices, or even with stable incomes and falling prices, provided only that unemployment is maintained at some " required level " which is by no means unreasonably large.

There are a number of things, in my opinion, which are fatally wrong with this conclusion. In the first place, for reasons analysed in my previous lecture, an under-employment equilibrium when the level of production is limited by demand, and not by production bottlenecks, is only stable when the level of production is stationary over time, so that there is no *growth* in output giving rise to induced investment. If effective demand is kept at a high enough level, and at a growing level, to induce a steady growth in capacity year by year, forces are inevitably generated which will push the economy to the point of maximum production in the short run: at any lesser level of output, as we have seen, the demand curve will cut the supply curve

[1] *Ibid.*, p. 299.

the wrong way round, and equilibrium will be unstable. For this reason it may not be possible to run a growing economy at half-cock, i.e., to keep it steady at a less-than-full-employment level of activity.

But even if by a combination of superb skill and cunning and the sureness of touch of a tight-rope dancer, the Treasury and the Bank of England succeeded in keeping the economy in a moving state of unstable equilibrium—succeeded, in other words, in maintaining a steadily growing real demand for goods and services, whilst never allowing the economy to run up against the bottleneck of full employment--it is a fallacy to believe that the mere fact that unemployment is not allowed to fall below some critical level will suffice to keep down the rise in wages and profits to some prescribed level. For the very fact that production is growing will mean that profits are growing; it is the growth in profits which causes wages to rise and thereby step up the rate of growth of profits and also of wages. It is not enough, therefore, to create unemployment in order to stop a wage inflation. Since in a capitalist economy rising production is closely interlinked with rising profits—it is impossible to have the one without the other—a policy of " damping down demand " can only succeed in stopping the inflationary spiral if it also brings to a halt the process of growth. A 5, or even 10, per cent. rate of unemployment is perfectly compatible with a profit-wage spiral so long as growth goes on; and recent history is strewn with attempts to bring monetary depreciation to a halt by measures which brought the growth of production to a halt, whether or not they succeeded in halting the rise in prices.

Fortunately there is no real, inescapable dilemma here. All that is necessary is to recognize that the proper way of dealing with inflation is to damp down, or restrain, the rate of increase in money wages *as such*, instead of damping down the demand for goods and services. If the increase in wages is slowed down, the growth of monetary demand will be automatically damped down too, and so will the rise in profits. Measures restricting the cost-push inflation coming from the side of the unions, unlike measures restricting the demand for goods, do not necessarily interfere with the real rate of growth of the economy.

A democratic community, unlike totalitarian countries, cannot stop wages from rising by the mere fiat of the Governmental authority, whether through freezing wages, compulsory arbitration, the imposition of a central authority with vetoing powers, or the like. But within our existing institutional framework it is possible to develop arrangements which would tend to slow down the growth of wages and profits without the use of compulsory powers. The most promising line of approach seems to me the introduction, on American lines, of a system of " wage-contracts " concluded for a definite period, say, for two years or longer. If by this means the period elapsing between the re-negotiation of wages could be lengthened, the annual rate of increase in wages would be reduced, simply because the rate of growth of profits would be slowed down if wages were held steady for longer periods. If wages were negotiated afresh after, say, the lapse of every *two* years, the biennial round of wage increases would be bound to be less than twice the annual round. It is with some such arrangement that the rise in wages could be brought down to the 4–5 per cent. annual rate which I mentioned earlier as a reasonable target.

RATE OF GROWTH OF PRODUCTIVITY

More intriguing is the question whether, and by what means, the rate of growth of output per head could be stepped up so as to make this " safe " level of wage and profit inflation of 4–5 per cent a year consistent with stable prices. (A faster rate of economic growth would of course be a good thing in itself quite apart from the objective of avoiding inflation !) On this issue, most people pessimistically assume that the rate at which productivity is growing over time is just one of the facts of nature. Yet the rate of growth of productivity shows wide variations between different countries and periods: it could hardly be regarded as a law of nature that the annual growth of productivity should be 2 per cent. in Britain, 3½ per cent. in America, 6 per cent. in Germany, Italy or France, and 10 per cent. in Japan, as was the case in the 1950's (not to mention the fancy figures that drift across the Iron Curtain). These differences in the rates of growth of production were of course strongly correlated with the ratio of gross investment to the gross national product; fast-growing economies invariably invest a higher proportion of their current product than slow-growing economies. A higher ratio of investment to output is therefore an essential precondition for stepping up the rate of growth of the economy.

Yet it would be a mistake to believe, I think, that we could double the rate of growth merely by doubling the share of investment in output. If this were true, investment would have tended to expand on its own, so to speak, through the operation of market forces. As I argued in the previous lecture, the technical dynamism of an economy, its capacity to absorb or assimilate technical change, sets a limit to the *useful* rate of investment; and there are several instances (e.g. Norway) to show that a Government-directed expansion of the investment coefficient may merely lead to a sharp rise in the capital-output ratio with only a moderate effect on the rate of growth of productivity.

The factors which determine the growth of productivity partly depend on the improvement of design of newly installed plant and equipment, and partly on the rate of disappearance of obsolete equipment which sets a limit to the extent to which new equipment can be usefully absorbed by the economy. These factors are interconnected: since the faster the rate of improvement in productivity on newly installed equipment, the faster the decline of prices relative to wages (the faster, in other words, the rise in *real* as distinct from *money* wages), which in turn determines the rate at which ancient equipment disappears from the production process, and the greater, therefore, the scope for the introduction of new equipment. Further, a higher rate of increase in *real* wages not only increases the rate of turnover of equipment, but it tends to enhance the rate of improvement of design on new equipment, since it gives stronger incentives for making the new equipment *more* labour-saving. It is for this reason that the rate of growth of productivity appears to be positively correlated with the rate of growth of working population, for when the employed population is increasing, the rate of absorption of new equipment is relatively high (the average age of equipment is falling); this in turn accelerates obsolescence, and hence makes room for a still higher rate of absorption of new equipment at the same time as it enhances the rate of improvement of design. The low rate of growth of productivity in Britain may thus have something to do with the near-stationariness of her industrial working population: this makes for a low rate of absorption of new equipment, a slow rate of growth in real wages, hence a low rate of obsolescence and weak incentives for design-improvement.

If this view is correct, a great deal could be done by measures designed to accelerate the rate of scrapping of obsolete equipment and the consequent release of labour which would make room for a higher rate of absorption of new equipment. This may be stimulated by the revival of price-competition, tending to eliminate inefficient firms and inefficient plant; it could also be stimulated by special tax measures, complementing accelerated depreciation allowances at one end by obsolescence taxes at the other end—by some kind of negative depreciation allowance on the employment of over-age plant, if this were feasible.

There is no time to probe more deeply into such problems. All I have been able to do here is to indicate some of the reasons for the belief that there is no real justification for having a fatalistic attitude concerning the rate of economic growth that could be attained in the British economy, given the proper diagnosis and given a purposive direction.

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The Relation Between Unemployment and the Rate of Change of Money Wage Rates: A Comment

By Guy Routh

Professor Phillips has attempted to show[1] that the rate of change of money wage rates is explained by the level of unemployment and the rate of change of unemployment. The demonstration is impressive, yet I cannot help feeling that he has not quite got the wage series into orbit, or, if it is in orbit, that it is revolving round the wrong planet. My contention will be that his statistical material is not appropriate for the support of his hypothesis; that different wage and unemployment series will yield significantly different results; and that, even if the imperfections of the material are disregarded, Professor Phillips reads more into his evidence than it can in fact sustain.

The Statistical Material

1.1. Professor Phillips describes, in his hypothesis, an institutionless world where wage rates move up quite rapidly when employment is high because employers are " continually tempted to offer a little above the prevailing rates to attract the most suitable labour from other firms and industries "; and where they move down, much more slowly, when demand for labour is low, because of the reluctance of workers to offer their services at less than the prevailing rates. The logic of the exclusion of institutional factors is clear. The contention is that the relationship between changes in wage rates and unemployment has remained constant for nearly a hundred years; thus, since the institutions have undergone formidable changes in this period, they cannot be admitted as co-determinants.

The wage rates described in the hypothesis are the *effective* rates[2] determined at factory level as employers and workers adjust themselves to their changing environment. For economics, these effective rates have a peculiar interest, precisely because they are sensitive to market conditions. And if it were possible to demonstrate that they have reacted in the same degree to similar market conditions throughout a period when trade unions have grown greatly in power, one might reasonably conclude that trade unions have no effect on market rates. But here we have a double difficulty: the series that Professor Phillips actually uses come from Bowley and Wood and the Department

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[1] " The Relation between Unemployment and the Rate of Change of Money Wage Rates in the United Kingdom, 1861–1957 ", *Economica*, November 1958.

[2] This is the term used by Professor Phelps Brown to distinguish the rates actually paid by employers from those laid down in collective agreements or wages council orders.

(later Ministry) of Labour, who constructed their indexes on standard rates—the creatures of trade unions and employers' associations, the institutions which Professor Phillips tacitly excludes—and the standard rates bear an inconstant relation to the effective rates that Professor Phillips postulates in his rationale. Bowley and Wood tried to make some allowance for the divergence of standard rates from the reality; for the period after 1910, no such allowance has been made.

1.2. What is remarkable about the standard rates is their stability in the face of changing conditions. Between 1875 and 1900 the standard rates for engineering fitters in eleven towns in the United Kingdom changed on average just over five times in each town.[1] In three of these towns they changed only once in this period. In most other trades local rates show a similar rigidity, and the fluctuations in the general indexes of wage rates for this period arise from the incorporation of many occupations in many districts[2] and from the effects of sliding scales for wage rates in coal and metal manufacture. With the rigidity of the district rates went the process of bidding effective rates up or down, which Professor Phillips describes, and whose effects are momentarily revealed by the earnings census of 1906.[3]

1.3. Bowley and Wood made some allowance for divergence between the institutionally-proclaimed standard rates and the effective rates applied in practice, by trying to measure changes in the mode. In an address to the Royal Statistical Society in 1906, Bowley said[4]:

> The question remains, what is the relation of the normal rates on which our [his and Wood's] index numbers are based to average earnings? It will be remembered that we have endeavoured to get the predominant rate, or the mode, in each occupation. . . . Where trade unions have been weak the mode is likely to be less definite and the average below the standard. . . . On the whole it seems probable that the trade union minimum was imperfectly recognised in 1860, and that it was then in many cases a maximum. During the last forty-five years the minimum has become more and more what it pretends to be, and is very generally the lowest rate paid to skilled labour. . . . At the same time rates have become concentrated; the standard maximum has become the mode. . . . The effect of this would be that from 1860 to, perhaps, 1875, normal rates would rise faster than the union rates, and since that date would in general coincide.

1.4. So Bowley and Wood corrected the trade union rates with information collected from employers' associations and other sources. They also brought their indexes into general agreement with changes in the mode shown by the earnings enquiries of 1886 and 1906. But no such adjustment has been made in the materials used by Professor Phillips after 1911. These latter indexes no longer claim to reflect changes in effective rates—the rates determined by individual employers—but measure changes in wage rates in national agreements, wages council orders and so on, whose relation to effective rates is by no means fixed.

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[1] Board of Trade, *Report on Standard Time Rates of Wages in the U.K.*, 1900, p. 139.

[2] Wood's and Bowley's engineering and shipbuilding index incorporated 26 occupations and 19 districts.

[3] *The Report of an Enquiry by the Board of Trade into Earnings and Hours of Labour of Workpeople of the U.K.* in 1906 shows, e.g., average rates for London compositors for a normal week ranging from 39s. 11d. to 79s. 1d. in different sections of the trade when the standard rate was 39s.

[4] *Journal of the Royal Statistical Society*, March 1906, p. 182.

For the period 1948 to 1957, Professor Phillips shows a remarkable accord between the December-to-December movement of the Ministry of Labour's wage rate index and the curve he has fitted to the 1861–1913 data. But a different picture emerges if average hourly earnings are substituted for standard rates. Table 1 repeats the first two columns from Professor Phillips' Table 1 and adds a third, the percentage change in average hourly earnings from year to year[1]:

TABLE 1

PERCENTAGE CHANGES IN WAGE RATES AND EARNINGS, 1948–1957 (DECEMBER TO DECEMBER)

	Wage Rate Index	Computed from the Phillips Equation	Hourly Earnings
1948	3·9	3·5	6·5
1949	1·9	4·1	3·5
1950	4·6	4·4	5·7
1951	10·5	5·2	10·1
1952	6·4	4·5	6·5
1953	3·0	3·0	5·0
1954	4·4	4·5	7·3
1955	6·9	6·8	8·8
1956	7·9	8·0	5·8
1957	5·4	5·2	5·8

Table 1 reveals a paradox that would seem to require an explanation: between the changes in rates determined by the institutions (which the hypothesis omits) and those yielded by the Phillips equation (for the relevant levels of unemployment) there is an impressive accord, an accord which is much less pronounced in the case of hourly earnings, though these reflect not the decisions of the institutions but the events of the labour market.

2.1. The wage rate index used by Professor Phillips for the period 1860 to 1911 is derived from G. H. Wood's "Index for Workman of Unchanged Grade"[2], which in turn was based on the remarkable series of papers read before the Royal Statistical Society between 1895 and 1906 by himself and/or Bowley. It has fourteen constituent indexes: three for agriculture (England and Wales, Scotland and Ireland),

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[1] *Ministry of Labour Gazette*, February 1959, p. 47. To approximate the figure for December each year, I have averaged earnings for the previous October and the following April.

[2] *Journal of the Royal Statistical Society*, March 1909.

and one each for building, printing, shipbuilding, coal, puddling, cotton, wool and worsted (Huddersfield), worsted (Bradford), gas, and furniture. There is some mystery about how these constituents were combined, and even Bowley had forgotten by the time he published his *Wages and Income* . . . in 1937, when he said of it: " It appears to be obtained by . . . applying fixed weights instead of allowing for the varying rates of growth of different occupations ".[1] Wood himself gives no clue beyond calling it an index of money wages " For Workman of Unchanged Grade " (not " workmen " as Bowley quotes it), and saying it should be regarded as the result of " taking a group of typical workpeople . . . and making no allowance for the constant tendency to leave the ill-paid occupations for the more remunerative ".[2] The movement of the " Unchanged Grade " index is in fact very similar to, though not identical with, that of the unweighted average that Wood gives for fourteen years between 1850 and 1906.[3] The differences are possibly due to some method of grouping the agricultural or textile indexes in the " Unchanged Grade " version.[4]

2.2. I shall return later to the question of weighting and show the difference that is made by substituting Wood's vari-weighted (allowing for changes in numbers employed) for his fixed-weight index. Here my doubt is whether *any* sort of unweighted or fixed-weight average is suitable as a measure of the effect of unemployment on wage rates for any period in which there have been substantial occupational and/or industrial shifts. If unemployment has a depressing effect on wage rates (and who would deny that it has ?), there would be a double motive for a migration from depressed to prosperous industries: one, for greater security of employment; two, for higher pay. It is aggregate results with which Professor Phillips is dealing, and aggregate wage changes are, in part, determined by the flow of workers from low-paid to higher-paid industries. The fixed-weight wage index fails to allow for this and so exaggerates the depressing effect of unemployment. Thus Wood's vari-weighted index shows a rise of 63 per cent between 1860 and 1910, compared with a rise of only 35 per cent in the fixed-weight index.[5]

3.1. The problem of weighting, both in the wage rate index and in the unemployment series, has been neglected by Professor Phillips, with resulting distortions in the apparent relation of aggregate wage movements to aggregate unemployment. Bowley remarked of his and Wood's general indexes that " the movement of the average is the resultant of very unequal changes in different occupations and industries ", and showed some constituent indexes.[1] Between 1860 and 1906 the increases in Wood's fourteen indexes ranged from 18 per cent (puddling) to 72 per cent (agriculture, Scotland). So it is that quite wide differences can be produced by differences in weighting or coverage. As between Bowley's and Wood's general indexes these differences are for most purposes not important, but, as Professor Phillips has himself instanced, they can be significant for present purposes.[2]

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[1] *Wages and Income in the United Kingdom since 1860*, 1937, p. 7.

[2] *Journal of the Royal Statistical Society*, March 1909, p. 99.

[3] *Ibid.*, Table 1.

[4] The weights could be calculated algebraically, using the data in Wood's Table 1 and Appendix.

[5] It may well be that Professor Phillips was aware of this point, for he seems to have been under the impression that he was using a vari-weighted chain index. See section 7.4 below.

3.2. The difficulties of finding a representative average are magnified when we come to unemployment. It may be that the nearest approach to a satisfactory solution would be to correlate wage rate changes in a general index with average unemployment for the same industries or occupations. In fact, in the two series used by Professor Phillips, neither the weights, occupations nor industries are a good match. The pre-1914 unemployment series is calculated from returns made to the Board of Trade by trade unions which operated unemployment funds. Agriculture, coal mining, railways, women and unskilled workers hardly affected the series. In mid-1881, the membership of unions reporting regularly was 140,000; in 1901 it was 531,000; and in 1914, nearly a million.[3] The method used in calculating average unemployment was to express each month the number of unemployed members as a percentage of total membership. One bias was the heavy, though relatively declining, representation of membership in engineering, shipbuilding and metal, which in 1860–70 accounted for about three-quarters of the total membership of the unions concerned. In 1881–90 this had fallen to about three-fifths and thereafter was about two-fifths. In these industries, victims of the accelerator, employment fluctuated within wider limits than in most other industries. As their influence became diluted, the statistics produced the illusion that fluctuations in employment were decreasing with the advance of the years. To remove this impression, the Board of Trade published in 1904 an average that gave equal weight to the unions in engineering, shipbuilding and metal, on the one hand, and to all other industries on the other.[4] Later, the figures for the period from 1894 were further revised and additional unions were included.[5] It is this weighted average that Lord Beveridge[6] and Professor Phillips have used.

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[1] *Wages and Income . . . since 1860*, p. 8. There is a strange error in this table, the index for agriculture being repeated under the heading, " Engineering and Shipbuilding ", the correct figures for which are in the *Journal of the Royal Statistical Society*, March 1906, p. 185.

[2] See Professor Phillips' Figures 4 and 4A. Bowley sets out the various indexes, *op cit.*, Table 1.

[3] See *Survey of Industrial Relations—Report by the Committee on Industry and Trade*, 1926, p. 218.

[4] See *Memorandum on British and Foreign Trade and Industrial Conditions*, Cd. 2337 of 1904.

[5] These were published in the *Abstracts of Labour Statistics*, up to and including the 15th.

[6] *Full Employment in a Free Society*, 1944, Table 22, pp. 312–13.

3.3. This simple weighting was applied not to make the average more indicative of general unemployment in any one year, but to remove the impression that fluctuations were decreasing. There was no other official attempt at weighting, because the figures were presented not so much as an absolute measure of unemployment but as an indicator of trends, a function that they seem to have performed satisfactorily. Their unreliability as a measure of unemployment for any one year is due to the wide dispersion of rates of unemployment between different industries and occupations, which meant that the calculated rate could be altered by leaving some unions out or bringing others in. In 1894, for example, when the unweighted average was 6.9 per cent and the weighted average 7.7 per cent, the dispersion at the end of the year was as follows: out of 67 unions with 368,000 members, 16 with 79,000 members had less than 3 per cent unemployed; and at the other extreme, 14 with 83,000 members had 10 per cent or more unemployed. Thus changes in coverage could occasion substantial changes in the recorded rate.

3.4. Lord Beveridge has tried to estimate the bias in the trade union unemployment figures as compared with the employment exchange data of the inter-war years[1]; and applies a series of adjustments that raise the average trade union unemployment rate for the period 1883 to 1913 from 4.8 per cent to about 6 per cent. This adjustment would entail a shift of the Phillips curve to the right. But this would still leave open the question of the magnitude of the fluctuations. It is possible, for instance, that the series may have exaggerated unemployment when it was high and understated it when it was low, and it may have done this in differing degrees at different times. If this were so, not only would Professor Phillips' six points (shown in his Figure 1) have to move to the right in accordance with the Beveridge adjustment, but they would also move relatively to one another. I return to this possibility in section 6 below.

3.5. For the period 1861–1913 coal and agriculture are heavily represented in the wage series used by Professor Phillips, but are virtually excluded from the unemployment series, which also excludes transport workers, unskilled workers and women. Bowley made an attempt to remedy these omissions and presented his results in a paper to the Royal Statistical Society in 1912.[2] Bowley's index of unemployment generally (though not always) moves in the same direction as the Board of Trade averages, but the fluctuations shown are generally of different amplitude. Presumably because of war and the introduction of unemployment insurance, Bowley did not pursue the exercise for the years after 1913, and there remains a great deal of work to be done before a definitive statistical series of employment or unemployment for this period is evolved. Thus, in short, Professor

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[1] *Ibid.*, pp. 328 *et seq.*

[2] "The Measurement of Employment: an Experiment", *Journal of the Royal Statistical Society*, July 1912.

Phillips is not on firm ground when he assumes that the pre-1914 unemployment series is consistent within itself over time, or that it is a measure whose scope is comparable with that of the subsequent series used by him.

4.1. How are the wage and unemployment series geared together? Professor Phillips suggests that it is the percentage change in wage rates over a year, i.e., from December to December, that is explained by the average percentage unemployment for that year[1] and the rate of change of unemployment in that year.[2] But for one period he uses first central differences for the wage series; and for the end-period he lags unemployment by seven months. Both these devices introduce inconsistencies in the time-relationship between unemployment and wage rate changes in the different periods which he compares.

4.2. The use, for the period 1861 to 1920, of first central differences as percentages of the central year, has been made on the assumption that Wood's wage index measures the average of wage rates during each year. Thus it might be assumed that the index number for each year measured the position roughly at mid-year, and that the difference between the numbers for two consecutive years measured the change between the mid-points of those years. How were these figures to be matched with those for unemployment, representing annual averages of monthly figures? If the index of wage rates shows for each year the position at mid-year, then half the change in wage rates between year 0 and year 2 would have occurred between 1st January and 31st December of year 1; and it is this that is measured by the first central difference.

4.3. But besides altering the incidence over time of the changes registered by the wage index, this device has a smoothing effect and thus introduces distortions of its own. As a measure of this distortion, I have lagged the unemployment series instead of advancing the wage series; i.e., by taking two-year moving averages, I have approximated a mid-year to mid-year average of unemployment. Table 2 shows the

TABLE 2
CHANGES IN WAGE RATES AND UNEMPLOYMENT, 1861–1913

	Ranges of Unemployment Percentages					
	0–2	2–3	3–4	4–5	5–7	7–11
Average unemployment percentage—						
1. Annual averages	1·52	2·35	3·48	4·49	5·95	8·37
2. Two-year moving averages	1·50	2·58	3·45	4·61	6·14	8·26
Average percentage change in wage rates:						
3. First central differences	5·05	1·54	0·85	0·67	−0·22	−0·34
4. Year-to-year changes	5·25	1·60	1·54	0·56	−0·52	−0·46

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[1] It is this correlation that is expressed in his curve.

[2] Professor Phillips does not give a measure of the rate of change of unemployment nor suggest with what lags it would affect wage rates.

results of associating these averages with percentage changes from year to year in the wage rate index used by Professor Phillips. Rows 1 and 3 show the co-ordinates of the six points to which Professor Phillips has fitted his curve; rows 2 and 4 show the results of synchronising unemployment levels and wage movements by lagging unemployment instead of by advancing wage rate changes. The use of first central differences can be seen to have had the effect of damping fluctuations in the rate of change of wage rates.

4.4. If Professor Phillips felt it desirable to use first central differences to measure changes in wage rates for the initial period, it would have been more consistent if he had used the same system for the later periods. Table 3 shows the effect of doing this for the period 1948 to 1957:

TABLE 3

PERCENTAGE CHANGES IN WEEKLY WAGE RATES

	1948	1949	1950	1951	1952	1953	1954	1955	1956	1957
1.	3·9	1·9	4·6	10·5	6·4	3·0	4·4	6·9	7·9	5·4
2.	3·8	2·3	5·1	8·0	6·2	4·4	5·4	7·2	6·2	4·1

Row 1: December-to-December changes used by Professor Phillips.

Row 2: Index of annual average rates, first central differences as percentages of central year.

The differences between the two series again illustrate the " non-neutrality " of the central difference device. The points in Professor Phillips' Fig. 11, if row 2 were substituted for row 1, would no longer " lie closely along a smooth curve which coincides almost exactly with the curve fitted to the 1861–1913 data ".

4.5. But to assume that the Wood indexes (and the Wood-Bowley indexes of which they are composed) represent wage rates averaged over each year is to attribute to them an exactness that they do not possess. In fact, they do not represent rates averaged over each year, but merely the average for the year of one sounding for each of the constituent indexes. Wood and Bowley were, as a rule, happy if they could identify the *year* of a change in a particular rate, without attempting to make allowances for the month in which the change occurred. So, when a change is registered from 100 to 102, it means that at some time in the year in point, wage rates rose 2 per cent, not that they rose 4 per cent on 1st July or 8 per cent on 1st October. Thus, then as now, the changes establishing the new levels may have been spread or clustered very differently over the span of different years,[1] and the Wood-Bowley indexes may best be regarded as recording the score at the end of each year. So it is inappropriate to advance the wage series on which the Phillips curve is based by the use of first central differences, and the correlation should be between annual percentage changes as shown by the wage index and annual average percentage unemployment.[1]

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[1] Professor Phillips neglects this circumstance, though it does seem a matter of some substance whether a new level is reached early or late in a year.

5.1. There are of course many differences in the unemployment and wage series in the various periods covered by Professor Phillips' study, some of which I have already mentioned. There is a further reason for being suspicious of the closeness of fit displayed in Professor Phillips' Figure 11 for the period 1948–57. On the wage side it is still wage movements that are measured[2]; but in calculating unemployment percentages, the divisor used is the total of employees, both wage- and salary-earners. If all occupational classes were equally prone to unemployment and to register at employment exchanges when unemployed, this would raise the divisor and the corresponding dividend in the same proportion and no harm would be done in comparing unemployment amongst all employees with unemployment amongst wage-earners; but the occupational tables of the 1951 population census show that the proportion of unemployed is much higher among wage-earners than among salary earners. In England and Wales 331,531 wage-earners were listed as unemployed and 64,117 salary-earners (2.44 and 1.38 per cent respectively). In Northern Ireland the figures were 37,610 wage-earners and 2,214 salary-earners (9.22 and 2.78 per cent). In each case the percentages for total unemployment (2.17 per cent in England and Wales and 8.17 per cent in Northern Ireland) would have to be raised by about one-eighth to establish the rate for wage-earners. Moreover, " the unemployed " in the official unemployment statistics are those who have registered at employment exchanges; and, because salary-earners are less inclined to register, there is a further understatement in the published averages.[3] The appropriate adjustment would have the effect of moving the points shown in Professor Phillips' Figures 10 and 11 to the right, once more dislodging those in Figure 11 from the hyperbola.

ALTERNATIVE CORRELATIONS

6.1. Even with the restricted material that is available for the period 1861 to 1913, various correlations are possible between unemployment and changes in wage rates. Professor Phillips has fitted his curve to six points that " give the average values of the rate of change of money wage rates and of the percentage unemployment in those years in which unemployment lay between 0 and 2, 2 and 3, 3 and 4, 4 and 5, 5 and 7, and 7 and 11 per cent respectively (the upper bound being included in each interval)." Table 4, below, shows the difference made to the co-ordinates of these points by selecting different sets of data. Rows 1.1 and 1.2 of the Table show the values taken by Professor Phillips: for unemployment, one-half of total weights to metal-using industries; for wage rates, Wood's fixed-weight index, first central differences as percentages of central years. Row 1.3 shows the effect of substituting Wood's vari-weighted wage rate index,[1] for purposes of comparison also in the form of central differences.

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[1] The effect of this on the co-ordinates of the " six points " is shown in Table 4 below, rows 1.1 and 3.2.

[2] Though no allowance is now made for divergence between standard and effective rates.

[3] For a more complete treatment of this subject, see H. A. Turner, " Employment Fluctuations, Labour Supply and Bargaining Power ", *The Manchester School*, May 1959, pp. 177 *et seq.*

In row 2.1, I have made a first attempt at getting a better match between the wage and unemployment series. Three out of Wood's 14 constituent wage indexes relate to the metal-using industries which, in 1881, gave employment to about one-fifth of the workers in the industries included in his index. One may then reconstruct the unemployment series giving a weight of one-fifth to this group, so reducing the disproportion of their influence. The number of years in the 0—2 per cent range of unemployment percentages is thus increased from 6 to 10; and in the 7 to 11 per cent range it is reduced from 9 to 2.

Rows 3.1 and 3.2 (repeated from Table 2, above) show the effect of applying two-year moving averages to the unemployment series (used by Professor Phillips) as an alternative to applying first central differences to the wage series. Both place them in the same relation in time, but there is a significant difference in the results.

Lastly, in row 4.1, I have applied a more radical reweighting to the unemployment series, to reduce the weight of the metal-using industries and also to give them increasing weight with the passage of time.[2]

6.2. The contrasts and similarities of the various correlations can be further illustrated by fitting curves to the six points in the same way as Professor Phillips has done.[3] For the new curve,[4] the procedure was to choose the value of a which gave the highest correlation coefficient for the regression

$$\log (y+a) = \log b + c \log x$$

taken over all six groups instead of only the first four (where y is the rate of change of wage rates and x percentage unemployment). Thus slightly different coefficients are obtained from the data Professor Phillips used: the present procedure gives

$$\log (y+0.900) = 0.971 - 1.318 \log x$$

$$\text{or } y = 9.354\, x^{-1 \cdot 318} - 0.900 \quad \ldots\ldots\ldots\ldots \quad (1)$$

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[1] Professor Phelps Brown and Miss Sheila Hopkins ("The Course of Wage Rates in Five Countries, 1860–1939," *Oxford Economic Papers*, June 1950) adjusted Wood's fixed-weight index to allow for reductions in working hours in the 1870's, and I have made the same adjustments in the vari-weighted index.

[2] In 1861 in the U.K. there were approximately 330,000 wage earners in engineering, shipbuilding and metal, out of a total of about 11m. wage earners. In 1911, the numbers were 1·9m. out of 16m.

[3] Professor Phillips describes his fitting procedure on p. 290 of his article.

[4] My thanks are due to Mrs. Muriel Hill and Mrs. Frances Hiron who did the calculations for this and the other equations in this article, and to Mr. D. A. Rowe who directed the operation.

Now when the vari-weighted wage index is substituted for the fixed-weight (i.e., Table 4, row 1.3 for row 1.2), the equation is

$$\log (y+0.700)=0.967-1.343 \log x$$

$$\text{or } y=9.268x^{-1\cdot 343}-0.700 \quad \ldots\ldots\ldots\ldots\ldots\ldots (2)$$

TABLE 4

AVERAGE UNEMPLOYMENT PERCENTAGES AND AVERAGE PERCENTAGE CHANGES IN WAGE RATES, 1861–1913

	Ranges of Unemployment Percentages					
	0—2	2—3	3—4	4—5	5—7	7—11
1.1. Average Unemployment ..	1·52	2·35	3·48	4·49	5·95	8·37
(Years in Range) ..	(6)	(10)	(12)	(5)	(11)	(9)
1.2. Average Wage Rate Change ..	5·05	1·54	0·85	0·67	–0·22	–0·34
1.3. Average Wage Rate Change ..	4·91	1·79	1·16	0·71	0·08	–0·17
2.1. Average Unemployment ..	1·52	2·52	3·31	4·49	5·63	7·56
(Years in Range) ..	(10)	(13)	(8)	(12)	(8)	(2)
2.2. Average Wage Rate Change ..	3·86	1·14	1·42	0·13	–0·24	–0·17
3.1. Average Unemployment ..	1·50	2·58	3·45	4·61	6·14	8·26
(Years in Range) ..	(5)	(12)	(8)	(10)	(11)	(7)
3.2. Average Wage Rate Change ..	5·25	1·60	1·54	0·56	–0·52	–0·46
4.1. Average Unemployment ..	1·5	2·6	3·2	4·3	5·8	—
(Years in Range) ..	(13)	(13)	(12)	(7)	(8)	(0)
4.2. Average Wage Rate Change ..	3·80	1·06	0·59	0·20	–0·80	—

BASES OF CALCULATIONS IN TABLE

1·1. Unemployment with ½ total weights to metal-using industries.
1·2. Wood's fixed-weight index of wage rates, first central differences.
(1·1 and 1·2 are the co-ordinates of Professor Phillips' six points.)
1·3. Wood's vari-weighted index of wage rates, first central differences.
2·1. Unemployment with 1/5th total weights to metal-using industries.
2·2. As 1·3.
3·1. Two-year moving averages, unemployment series as 1·1.
3·2. Wood's fixed-weight index, year-to-year change.
4·1. Unemployment with weights of 1:20 to metal-using industries in 1861 and 1:4 in 1907, with five intermediate changes.
4·2. Wood's vari-weighted index, year-to-year change.

6.3. When a curve is fitted to the data in rows 3.1 and 3.2 (two-year moving averages for unemployment and percentage year-to-year change in the fixed-weight wage index) the equation is as follows:

$$\log (y+2.450)=1.005-0.821 \log x$$

$$\text{or } y=10.116x^{-0\cdot 821}-2.450 \qquad (3)$$

This is shown as curve B in the diagram on p. 311 below.

6.4. The greatest contrast, of course, occurs in the data in rows 4.1 and 4.2 of Table 4, when the equation for the fitted curve is

$$\log (y+3.350)=0.970-0.721 \log x$$

$$\text{or } y=9.325x^{-0\cdot 721}-3.350 \qquad (4)$$

This is shown as curve C in the diagram.

6.5. The diagram shows the Phillips curve (curve A) alongside curves B and C mentioned above. The stories told by these curves have important similarities and important differences. Their obvious similarities rest upon the fact that they are hyperbolas, so chosen to meet the requirements of Professor Phillips' hypothesis: large changes in wage rates are associated with small changes in unemployment when unemployment is low, and large changes in unemployment with small changes in wage rates when unemployment is high.[1] But this sort of association is in general confirmed by the ten-entry moving averages, also shown in the diagram and described in section 7.1 below, whose shape is not determined by any preconceptions.

6.6. There are also some significant differences between the curves. In the 53 years up to 1913 the average change (sign ignored) registered by Wood's vari-weighted index (with hours adjustment) was 1.8 per cent; thus a calculus associating unemployment with changes in wage rates must work within very narrow limits if it is to say anything worth saying. If it predicts a rise of 2 per cent in wage rates, in view of the level of unemployment, and, in the event, the rise is 1 per cent, the error in the prediction has been not 1 per cent but 100 per cent.

Yet even when the data used by Professor Phillips are rearranged only by lagging unemployment instead of advancing wage rate changes, differences appear that, in this context, are significant. Table 5 gives a selection of readings from the various equations.

In equation (1) (the Phillips equation), stable wage rates are associated with between 5.4 and 5.5 per cent unemployment; in equation (2), where the vari-weighted wage index is substituted for the fixed-weight index, they are associated with unemployment of between 6.8 and 6.9 per cent, or, in a working-force of 20 m. employees, an additional 280,000 unemployed.

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[1] See K. G. J. C. Knowles and C. B. Winsten, "Can the Level of Unemployment Explain Changes in Wages ?", *Bulletin of the Oxford University Institute of Statistics*, May 1959, p. 118, fn. 1.

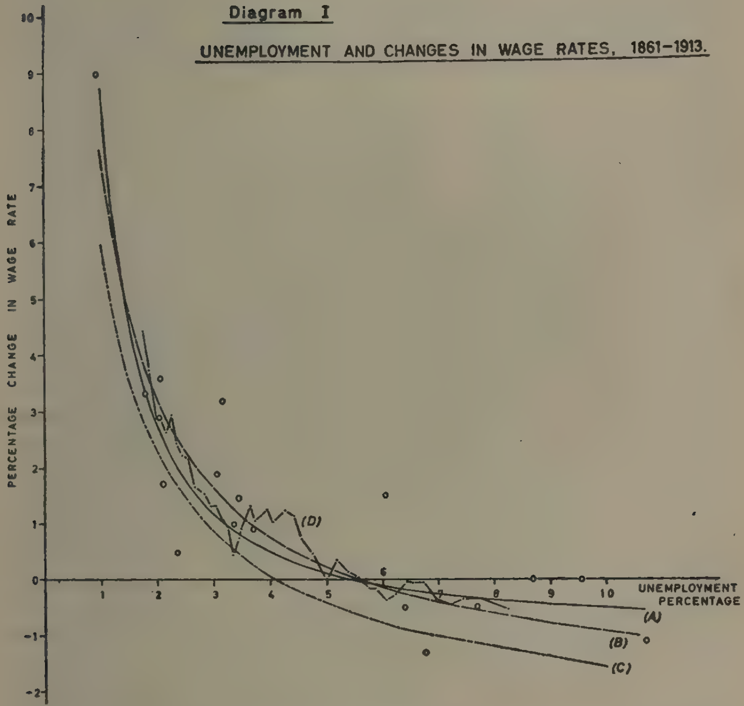

Diagram I

UNEMPLOYMENT AND CHANGES IN WAGE RATES, 1861–1913.

KEY

Relation between unemployment percentage and percentage change in wage rates:

(A) Equation (1), the Phillips equation
(B) Equation (3)
(C) Equation (4)
} For details, see pp. 308, 310.
(D) Ten-entry moving averages; years ranked in the order of unemployment percentages. (See p. 312.)

° Wage rate changes and unemployment for each of the 18 years of indeterminate trend in unemployment. (See pp. 314–315.)

TABLE 5

WAGE RATE CHANGES ASSOCIATED WITH VARIOUS LEVELS OF UNEMPLOYMENT: FOUR ALTERNATIVE EQUATIONS

Un-employment	Associated Changes in Wage Rates (Percentages)			
%	Equation (1)	Equation (2)	Equation (3)	Equation (4)
1	8·74	8·57	7·67	5·98
2	2·77	2·95	3·28	2·31
3	1·18	1·42	1·65	0·87
4	0·49	0·74	0·79	0·08
5	0·12	0·37	0·21	–0·43
6	–0·11	0·13	–0·13	–0·79
7	–0·26	–0·02	–0·40	–0·05
10	–0·51	–0·28	–0·92	–1·58

INTERPRETATION OF THE EVIDENCE

7.1. Leaving aside objections to the material, it seems to me that Professor Phillips has exaggerated the closeness of the relation that it reveals. The double process of simplification (first, the averaging by which the six points are derived, then the elimination of the remaining irregularities by the curve-fitting) has produced an effect of smoothness that is largely illusory. In the diagram, the jagged line (D) is derived by ranking the years 1861–1913 according to level of unemployment (using the same data as Professor Phillips) and calculating ten-entry moving averages of unemployment and associated wage rate changes. (Wood's vari-weighted index, year-to-year percentage change.) The negative correlation is in evidence but, with the smaller suppression of irregularities involved, the curve gives warning of the looseness of the relation between the two variables.

7.2. The average change in wage rates (sign ignored) given by the Phillips equation for the various levels of unemployment from 1861 to 1913 is 1.46 per cent. If the changes given by the equation for each year are related to those registered by the first central differences (as percentages of the central year) of the fixed-weight index in this period, the standard error is 1.29. If we assume, for simplicity, a normal error distribution, we should expect that about two-thirds of the actual changes would fall within a range of ±2.58. These limits would clearly be rather wide if it were not for the fact that some degree of error is explained, indeed required, by the second part of Professor Phillips' hypothesis: that when unemployment is falling, wage rate changes will be above average, and that when it is rising, they will be below average. It is this factor that produces the loops that are (so it appears to me) the most significant part of Professor Phillips' demonstration. Thus, points where unemployment is rising should be below the curve and points where unemployment is falling should be above the curve, and, in the case of the data used by Professor Phillips, I count only four exceptions to this rule. It is the "neutral" years (years not on an upward or a downward trend) that refuse to obey the rules. But I return to this point below.

7.3. But the loops that result from this process display some irregularities and Professor Phillips, who I think makes excessive demands of them, looks for disturbing factors to explain why this should be so. In his Figure 4, when the curve for the years 1879 to 1886 is in part lower than it ought to be, he says, " It seems possible that some peculiarity may have occurred in the construction of Wood's index for these years "; and he uses Bowley's index, which gives a better loop, in Figure 4A.

The difference between the two indexes arises in the years 1880–83, when Bowley's shows a rise of 3 per cent and Wood's one of only 1 per cent.[1] The constituent parts of Wood's index for 1880 and 1883 may be seen in Table 1 of his article of 1909,[2] while Bowley has published some of the constituents of his.[3] Wood's average is pulled down by a fall of 13.6 per cent for puddling, an industry not included by Bowley, and by a rise of only 7.4 per cent for coal, compared with Bowley's estimate of 12.0 per cent. The effect of a rejection of Wood's index in favour of Bowley's at this point is to exclude puddling and to allocate a higher increase to coal. But this treatment would call for the exclusion of puddling throughout; and so volatile is the puddling wage index, that its exclusion would make a considerable difference to Wood's fixed-weight index and thus to the six points and thus finally to Professor Phillips' hyperbola. And it can scarcely be claimed that wage rates in the coal industry, as measured by Wood's index, had not risen to a degree appropriate to the level of unemployment, because their rise, as measured by Wood, was well above the average. Professor Phillips' Figure 4 is simply indicative of the fact that the relationship between unemployment, unemployment changes and wage rate changes is less determinate than he claims and than his material is able to demonstrate.

7.3. For the period shown in his Figure 6 (1893–1904), Professor Phillips comments on the slow rise in wage rates: " This suggests that there may have been exceptional resistance by employers to wage increases from 1894 to 1896, culminating in industrial strife in 1897 "; and he refers to a rapid growth of employers' federations and the engineering strike and lock-out which lasted until January 1898. But Bowley and Wood's engineering and shipbuilding index moves from 108 in 1894 to 112 in 1896, and 117 in 1898, a percentage change well above the average, while Wood's Table 1 shows that it was again coal and puddling that pulled the average down by moving strongly against the trend between 1891 and 1896, when coal wages fell by 12.9 per cent and puddling wages by 9.7 per cent. Again, the relationship is more random than Professor Phillips supposes.

7.4. Professor Phillips (on page 292) suggests a reason for the narrowing of the loops in the later years of the period 1861 to 1913, as evidenced by his Figures 1 to 8. Referring to the linking of wage rates to product-prices in the coal and steel industries before the first world war, he says: " During the earlier years of the period these industries would have fairly large weights in the wage index, but with the greater coverage of the statistical material available in later years the weights of these industries in the index would be reduced ". But in Wood's fixed-weight index, which he uses, the weights were in fact fixed. The observed results may be due to imperfections in the statistical material, but more probably they are due to irregularity in the connexion between wage and unemployment changes.

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[1] Bowley, *Wages and Income . . . since 1860*, Table 1.
[2] *Journal of the Royal Statistical Society*, March 1909.
[3] *Op. cit.*, Table 2.

7.5. For the period 1913 to 1948 the relationship shown for the earlier period disappears; the loops go into reverse or cross and recross one another. For 1953–57 the points are again on the wrong side of the hyperbola when the data are used without adjustment. Without explanation, Professor Phillips introduces a lag of seven months in the response of wage rate changes to unemployment.[1] This has the effect of moving the points for these years on to the curve and thus abandons the second (and most suggestive) part of his hypothesis: the stimulating or tranquillising effect of *changes* in the level of unemployment, which produce the loops and explain the size of the standard error. Either the second part of the hypothesis is disproved, or the points have no business on the hyperbola. But in any case, as I have mentioned above, the unemployment series for this period is not consistent with that for 1861–1913.

7.6. This brings me to Professor Phillips' conclusion, which appears to me to be inadmissible. He has fitted his curve to six points, representing average wage rate changes when unemployment was between certain levels, and argues: "Since each interval includes years in which unemployment was increasing and years in which it was decreasing the effect of changing unemployment on the rate of change of wage rates tends to be cancelled out by this averaging, so that each cross gives an approximation to the rate of change of wages which would be associated with the indicated level of unemployment if unemployment were held constant at that level ".[2] From this follow his conclusions, important for public policy, that stable product prices (assuming an annual increase in productivity of 2 per cent) would be associated with a little less than 2½ per cent unemployment and stable wage rates with about 5½ per cent unemployment. This is consistent with the Phillips equation; I have shown that there are other equations, in some ways more valid, that would give different results. But it seems to me that it is inappropriate to use any of the equations for drawing conclusions of this sort.

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[1] An investigation by Miss Beryl Swift at the National Institute of Economic and Social Research shows that in 27 major negotiating groups the average time between the submission of a wage claim and its settlement in the ten years since June 1949 has been 117 days. There were wide variations in different years, e.g. 1953, 146 days; 1956, 95 days.

[2] In fact, the incidence of years of rising and falling unemployment is far from symmetrical in the six intervals.

The nearest approximation to years of stable unemployment in the period 1861–1913 are the years that represent turning points—years immediately preceded and succeeded by years, both of which had higher or lower unemployment percentages than themselves and in which, especially considering seasonal fluctuations, it would be difficult to decide whether unemployment had been rising or falling. The relationship of unemployment and wage rate changes for these years is shown in the diagram. Only two out of the eighteen points are on the Phillips curve. They indicate that in years when no decisive trend is shown in unemployment, a wage increase of 0.5 per cent may be associated with 2.35 per cent unemployment and a wage increase of 1.5 per cent may be associated with 6.05 per cent unemployment.[1] These values are far removed from those given by the Phillips equation.

Thus, while I go some of the way with Professor Phillips, I feel he has erred in failing to make any estimate of error. Nor has he taken into account the necessary imperfections of his data that produce not only random errors but observable distortions in the curves.

7.7. There are other reasons for rejecting the suggestion that a stable level of unemployment would go with annual changes in wage rates of constant proportion. There seems to be a tendency, evident in Professor Phillips' diagrams, for reductions in wage rates to exhaust themselves even though unemployment continues to increase. The same applies in the opposite direction when, after a few years of rising trend, wage increases appear to lose their impetus even though unemployment continues to fall. Just as lags may not confine themselves to a single calendar year, so expectations, in the real world, do not conform to the requirements of static analysis. So the belief that expectations, when unemployment appeared to be stable, would be mid-way between what they would be with rising unemployment and what they would be with falling unemployment, seems to me unwarranted. To assume that when unemployment is stable, wage rate changes can be read from the curve, is to assume away the effect of lags and expectations.

8.1. Changes in wage rates depend on a complex of conditions, of which the rate of unemployment is one of the best available indicators. But the same rate of unemployment may indicate different things at different times, depending, as it does, on technological, structural and regional changes in industry, as well as on cyclical movements from prosperity to depression. Institutional factors, too, will determine how far workers are able to take advantage of favourable conditions or defend themselves against unfavourable ones. In these circumstances, unemployment can be taken as an indicator of probable wage rate changes only within wide limits; for more accurate forecasting, other, including institutional, factors must be taken into consideration, and this some econometricians have tried to do.

National Institute of Economic and Social Research.

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[1] These figures are taken from the data used by Professor Phillips.

Welfare Economics, Ethics, and Essentialism[1]

By G. C. ARCHIBALD

In this paper I shall propose the apparently heretical view that welfare economics does not depend upon a foundation of value judgments. I shall argue that the common,[2] contrary, opinion is the consequence of a muddle; and I shall claim that my own opinion is consistent with the classical Robbinsian distinction between positive and normative economics. Throughout this paper, " welfare economics " means " the new welfare economics ". I thus take it for granted that our welfare economics is Paretean in the sense that all criteria involving interpersonal comparisons of utility are avoided; the argument does not apply to " old ", or Pigovian, welfare economics. The argument is, first, that the theorems of the new welfare economics need not be prefaced by judgments of value and, second, that attempts to base the new welfare economics on judgments of value only cause unnecessary difficulty and confusion. The argument will not satisfy those who, because they demand of welfare economics that it " tell them what to do ", *understand* by welfare economics a discipline necessarily founded upon value judgments, and therefore assert simply that my use of the term is not theirs. The demand cannot be satisfied, and the attempt to found the discipline upon value judgments only causes unnecessary trouble; but there may be a terminological problem. If terminology causes difficulties, the reader may substitute some other term, such as " type A economics ", wherever I have written " welfare economics ", provided that he understands the term to be the collective one for those investigations that are commonly labelled " the new welfare economics ".

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[1] This paper is based upon one circulated to the members of Professor Robbins' Seminar at the London School of Economics in February 1958. I am indebted to the members of the seminar for stimulus, comment, and criticism which has led to substantial revision. I also had the pleasure of reading a paper on this subject to the Economics Society of the London School of Economics, to the members of which I am indebted for the opportunity, and for their criticism and encouragement. Some of my friends, who at first disagreed with my view, announced after long argument that it was true " but trivial ". The length of the argument preceding their conversion is my excuse for now setting the matter out in full. In preparing the present version I have been greatly assisted by the comments of Professor Lionel Robbins, Dr. J. Agassi, Mr. K. Klappholz, Dr. R. G. Lipsey, and Dr. E. J. Mishan, none of whom may be held responsible for what is actually said here.

[2] A complete list of those writers who have maintained that welfare economics depends upon some minimum value judgments would be tedious. The view has recently been expressed, however, by I. M. D. Little, *A Critique of Welfare Economics*, 2nd edn., 1957, and J. de V. Graaff, *Theoretical Welfare Economics*, 1957, whose arguments are discussed in detail below. The same view has been adopted by E. J. Mishan (" A Re-appraisal of the Principles of Resource Allocation ", *Economica*, November 1957, p. 325, and " An Investigation into some Alleged Contradictions in Welfare Economics ", *Economic Journal*, September 1947, pp. 446 and 452), with whom I have had the benefit of discussion. Other authors, of course, have taken the view expressed here, but for the most part without discussion. An exception is Hla Myint, *Theories of Welfare Economics*, 1948, who defends the position taken here (see particularly Chap. VII, Sec. IV, Chap. VIII, Sec. III, and Chap. XXI) against arguments which we need not consider again.

The order of the argument is as follows: I shall first set out a brief statement of my own view; I shall then enquire into the causes of the difficulty and the main arguments of the modern welfare economists with whom I disagree; and I shall show how some of the principal theorems of welfare economics stand when they are properly understood. To avoid misunderstanding, I wish to warn the reader explicitly that nothing I say should be understood as implying that any prescription whatever can be made without value judgments. The argument has, however, important implications for the way in which we proceed from welfare theory to prescription.

* * * *

" Economics is not concerned with ends as such. It assumes that human beings have ends . . . and it asks how their progress towards their objectives is conditioned by the scarcity of means. . . ".[1] If this is accepted as a description of positive economics, as I think it is, it is hard to understand why welfare economics should be set apart. If we enquire into the efficiency of alternative arrangements for satisfying *given* wants,[2] why is a judgment about these wants a necessary foundation for the theorems we discover? Why, for example, is an enquiry into the effects of trade upon the world's production frontier an ethical exercise, while an enquiry into the effects of investment upon the level of employment is not? It appears already that the burden of proof rests with those who support the common view.

The sensible procedure in welfare economics appears quite simply to be this: we take, as an interesting criterion, the choice-system of the individuals, and ask how different arrangements alter the available choices. That we call the choice-criterion an index of welfare is purely a definitional shorthand, a convenience. It carries with it no value judgment or prescriptive implication. We could as well say, in employment theory, that the level of employment was to be rechristened the index of welfare—we should have altered nothing of substance by this terminological rearrangement (although we might have introduced a good deal of unnecessary confusion). The welfare index has been admirably explained by Graaff:[1]

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[1] Lionel Robbins, *An Essay on the Nature and Significance of Economic Science*, 2nd edn., 1935, p. 24. But cf. pp. 90–91, and 141–42, for a view of welfare economics contrary to that expressed in this paper. Robbins' view I believe to have been correct given that welfare economics depended upon (untestable) inter-personal comparisons of utility, but not otherwise.

[2] On the position of welfare economics, and the definition of the economic problem, Myint writes (*op. cit.*, p. 117): " Once that definition is accepted, it will be seen that the economist cannot be content with the mere description of how the economic system works; by the very definition of his problem, he is obliged to consider the further question how efficiently the economic system fulfils its function of adjusting the means to *given* ends ". Later he adds (p. 199): " . . . welfare analysis need not involve any normative value judgments so long as we take the wants of the individuals to be *given* and *constant* and confine our study to the purely mechanical efficiency of the economic system in satisfying these given wants ". Myint's problem is the important one of whether we can take the wants as given and constant.

> " The matter can be put somewhat formally by saying that a person's welfare map is *defined as identical* with his preference map—which indicates how he would choose between different situations, if he were given the opportunity for choice. To say that his welfare would be higher in *A* rather than in *B* is thus no more than to say that he would choose *A* rather than *B*, if he were allowed to make the choice."

The mystery is why Graaff, or anyone else, should believe that we must say " this is good " before we can conduct an inquiry into the arrangements that will allow the individual the choice of *A*. How *does* this enquiry differ from Robbins' ? We must now start our enquiry into the causes of the muddle.

* * * *

The usual line of argument seems to start from the presumption that " welfare " is a concept clear to our intuitions, and that the problem is to find a suitable measure for it. Thus Graaff, in spite of the paragraph quoted above, thinks it necessary to lay " the foundations for welfare analysis at the *subjective level*, by inquiring into the meaning we can attach to phrases like ' quantities of satisfaction ' or ' utility '."[2] He also argues that, in welfare economics, we test the assumptions, " factual and ethical ", rather than the conclusions.[3] The testing of " ethical assumptions ", however, necessarily involves introspection and intuition. Little's position, similarly, seems to be that we start by finding definitions which accord with common usage, and will be widely acceptable.[4] This means, again, that the starting point is our intuitive notion of welfare, and that the first task of the welfare economist is to sort out and formalize the definition which best corresponds to that intuitive notion.[5] We then obtain a " widely acceptable " measure or index of welfare, and are left with the technical problem of how to increase it.

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[1] *Op. cit.*, p. 5. Italics mine.

[2] *Ibid.*, p. 33; italics in original. Contrast his remark, p. 2, that economists might " . . . train themselves to strip terms of their normative associations, and learn to regard the ' national income ' as no more than ' that which this index measures '—in the same way that positivist psychologists define ' intelligence ' as ' that which intelligence tests measure ' ". His reasons for rejecting this excellent procedure, which seem to be singularly weak, are discussed below.

[3] *Ibid.*, p. 3. Testing welfare conclusions is discussed below.

[4] See the whole of his Chapter V, " Value Judgments and Welfare Economics ", which is considered in greater detail below.

[5] Or best corresponds to the intuitive notion which he believes to be most widely held.

Now this procedure—we search our intuitions for the best definition of welfare, we find out how welfare may be increased, and we conclude that we now know what we ought to do—is open to serious methodological objection,[1] and is at the bottom of all the trouble and misunderstanding in welfare economics. The crucial point is how we approach the matter of definitions. There are two approaches:[2]

1. We start by defining our concepts, trying to find a suitable formal expression for our intuitions, or for what we understand common usage to be. If we have the correct definitions, and if our deductions are logically valid, then our conclusions are true.
2. A definition is nothing but a convenient shorthand phrase. On every count except linguistic convenience, we could get on as well without it. Thus the term defined is the left-hand side of an identity. Graaff's definition of the welfare index, quoted above, can easily be rearranged as an example:

> " welfare has increased " $\equiv$ the individual chooses A rather than B when both are available; only B was available; both A and B are now available.

The first approach, that of Plato and Aristotle, Professor Popper calls essentialism, the second nominalism.[3] There are two main objections to essentialism:

1. It leads to the well known problem of the infinite regress. In defining one word we use others; we are challenged to define them, and use others again, and so on. Economists who have started by trying to define " welfare " have commonly ended by trying to define " economic " and " goods ".

2. In the event of disagreement, appeal can only be made to the intuition. If different men's intuitions disagree, there is nothing more to be said, no court of appeal. Given the starting point, if the logic is correct the result is universal truth. And we can only question the starting point by saying " That's not how I should define welfare ". Reply: " It suits me ". This is an argument of the " It is: it isn't " variety, to avoid which Plato introduced his theory of ideal forms. But then we are left with the question: how do we know when the wise man has correctly perceived the ideal form?

Consider the trouble we are saved by being nominalists in our welfare economics! We need not encumber ourselves with metaphysical inquiries into the " true meaning " of " happiness " or

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[1] Little, *Critique*, p. 1, makes an extraordinary statement: " Economists have used no methods of scientific research in arriving at their conclusions about economic welfare; and since there are no methods of scientific research involved there can be no methodology ".

[2] In what follows here I attempt a condensed statement of Professor Popper's views. See his *The Open Society and its Enemies* 1945, particularly chap. III, " Plato's Theory of Forms or Ideas ", chap. XI, " The Aristotelian Roots of Hegelianism ", and the accompanying notes.

[3] In the text I have simply used the terms essentialism and nominalism for what Popper calls *methodological essentialism* and *methodological nominalism*.

" satisfaction " or " utility ". We need not preface our theorems with irrelevant value judgments. Instead, we proceed directly to our enquiry: under what arrangements will this individual's choice be expanded ? What are the necessary conditions for a situation in which no one's choice may be expanded without a contraction in someone else's ? What are the necessary conditions for a situation in which the output of no good can be increased without a reduction in the output of another ?[1] We may speak of individual welfare and Paretean optima, but these terms add nothing except the convenience of a shorthand. We might often adopt a device of Carnap's,[2] and speak of " welfare_1", " welfare_2", etc., when we are using different criteria in close conjunction. Thus we might avoid the palpably essentialist question about the compensation tests: has *actual welfare*[3] increased if compensation is not paid ? International trade offers another convenient example:[4] free trade, by some criteria, maximizes " world welfare "; non-free trade can maximize " one country's welfare ". Will we ask which maximizes " true welfare " ?[5]

The position I advocate may be summarized. The enquiries we label " welfare economics " are positive enquiries into the effects on certain indexes of alternative arrangements. On the nominalist view of definitions, replacing the full statement of the indexes and criteria with words such as welfare is purely a matter of convenience. No value judgments need precede the enquiry; none is implied when a short word is substituted for a long explanation; and the conclusions have no prescriptive force. The theorems of welfare economics are thus theorems in positive economics; they are concerned with the relationship between *given* ends and available means. Thus welfare theorems do not differ from theorems about, e.g., how full employment may be achieved. We have a single dichotomy in economics, between positive enquiries into how something may be done, and normative recommendations that it should be done. As Radomysler said:[1]

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[1] Graaff, *op. cit.*, p. 14, prefaces this enquiry with the statement that " the only value judgement required in the analysis is that more of any output, or less of any input, is *cet. par.* a good thing ". Why this judgment should be necessary to the *analysis*, he does not explain. We have an enquiry into the necessary conditions for satisfying a certain relation: that is all. The great merit of Graaff's book, and the reason why it is so frequently quoted here, is that the theorems are set out with such lucidity and freedom from spurious value implications, that the irrelevance of the preceding value judgments is readily apparent. If a few passages, such as the one quoted above, were struck out, nothing else in the book would suffer or require amendment. See also p. 324, note 1, below.

[2] Rudolf Carnap, " The Two Concepts of Probability ", in *Readings in the Philosophy of Science*, ed. Fiegl and Brodbeck, 1953. I think Carnap underestimates the number of concepts of probability that have been discussed; but I wish to draw attention to his method of sorting out a muddle.

[3] Both Graaff and Little use this term, although both show all that is necessary without it: that an action may change different indexes differently.

[4] The controversy over the valuation of the national income is yet another example. Here Graaff's conclusion, pp. 164–66, seems to me to be perfectly consistent with the view expressed here.

[5] The illustration can be pushed further. Non-free trade may increase, say, farmers' welfare, and diminish that of industrially employed consumers. We may judge this to be good, or bad; but we will not ask if it increases (decreases) " true welfare ".

> " As the ' economics of welfare ' is concerned with the causes of welfare, it follows that it is a positive study, and not a normative study of what ought to be done.
>
> " The study of the causes of welfare and prescription are not far apart, the former to most people is only a preparation for the latter. Nevertheless, they are two different things."

They are, that is to say, logically distinct, however closely they are associated in our interests. They are distinguished by the fact that prescription must be preceded by a value judgment such as " we should try to increase welfare as measured by this index (rather than that one) ". Similarly, positive and normative are separated in employment theory by a value judgment such as that " we should try to maintain full employment (rather than, e.g., stable prices) ".

* * * *

It seems that all this is very simple, and saves a lot of trouble. But objections have been made which I must now try to state and meet.

1. It is sometimes argued that, by the mere act of selecting an index, calling it a welfare index, and taking it so seriously as to conduct a lengthy enquiry into the means of raising it, we imply a value judgment. The error here lies in not perceiving that the statements " this is interesting " and " this is good " are not identical. An index may be particularly interesting to us because we think it good (or bad), but a value implication does not follow from a declaration of interest. And there are many other reasons, besides our own value judgments, for finding an index interesting: it may be important to other people; it may raise interesting technical questions; it may appeal in many ways to our curiosity without having our moral approbation. And even if it has, nothing follows. Does employment theory become normative if Keynes was distressed by unemployment? It is similarly suggested that, whatever our pretensions to the contrary, we cannot, in fact, avoid making judgments about ends. We refuse to enquire into the conditions for maximizing misery, or the birth-rate, and, by this selection among ends, we show that our value judgments are influencing our enquiries from the beginning. Here again is the confusion between " interesting " and "approved ". The statement " this is good " is not a logical implication of the statement " this is worth investigating ", although " this is interesting " is generally suggested by " this is good ".

In any case, the circumstance that our selection of subjects for investigation frequently reflects our value judgments in no way distinguishes welfare economics from any other discipline. The choice of problems for, e.g., medical research, may well reflect judgments about what is important to health and happiness. Bacteriology is not on this account a normative study.

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[1] A. Radomysler, " Welfare Economics and Economic Policy ", *Economica*, August 1946, p. 199, quoted by Little, *op. cit.*, p. 78, as an example of the view with which he disagrees.

2. It sometimes appears to be suggested that, because the criterion in welfare economics is the individual's utility function, the conclusions have, in some sense, a moral superiority to conclusions about other criteria such as employment or income distribution. This view is hard to deal with because it is not made explicit; but I suspect that it underlies some writings on the subject. (The reader who believes that I am seeing ghosts may ignore this paragraph without losing anything of importance to the main argument.) It seems to be suggested that there is a peculiar virtue in the utility-function criterion[1] which is implicitly recognized in welfare economics, and which is absent from other criteria. The point is simply that, whatever virtue we may find in the criterion when we are considering our choice of policy, it is irrelevant to the analysis. It may heighten our interest in welfare economics; but that is all. In any case, we can formulate the welfare theorems in terms of expanded choice,[2] and do not require utility constructions. The principle of parsimony suggests that, if we can get rid of such constructions, we should; and this is all the more desirable

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[1] We may notice here a somewhat curious problem of the conventional approach. It is usual to admit that utility functions may be interdependent, in the sense that Jones' welfare may depend not only on the choice of goods available to him, but also on the choice available to Smith. This interdependence is then ignored in a treatment which purports to be based on the value judgment " individuals are the best judges of their own welfare; and, as liberals, we always accept their verdict ". Thus the conventional welfare economist appears to be saying either that the utility functions are not interdependent after all, or that they are, but should not be. The latter position is patently inconsistent with the " liberal " value judgments on which the whole argument is alleged to depend (for Mishan, " Re-appraisal ", p. 324, " rational " individuals are those whose utility functions are independent). These problems are discussed in W. J. Baumol, *Welfare Economics and the Theory of the State*, London, 1952. One of Baumol's illustrations, p. 127, note, deserves to be better known. Suppose that a conscript can hire a substitute. Both conscript and substitute enjoy the " gains from trade ". A third party declares that the arrangement reduces his welfare, and votes for its legal prohibition. What does the welfare economics founded upon the " ethical judgment " that people should be free to maximize (independent) utility functions, subject only to the budget restraint, make of this? (A notable feature of some welfare writings has been the prejudice that there is more virtue in a choice made in the market than in one made in a polling booth.) These problems disappear when we recognize that welfare economics *tells us only about changes in the choices available*, and tells us nothing about what people ought to feel or what we ought to do.

[2] I am indebted to Dr. R. G. Lipsey for demonstrating to me that the theorems of welfare economics can be expressed without utility functions or preference maps. A remarkably simple statement of revealed preference demand theory will be found in K. Lancaster, " Revising Demand Theory ", *Economica*, November 1957 particularly p. 356, note; and a statement of welfare theorems in terms of this demand theory in the same author's " Welfare Propositions in Terms of Consistency and Expanded Choice ", *Economic Journal*, September 1958. In Little's analysis, indifference curves are replaced by behaviour lines, and " ' on a higher behaviour line ' *entails* ' in a chosen position ' " (p. 38); but Little does not exploit the advantages of the choice-criterion. In Graaff's analysis, as we have seen, an increase in welfare is synonymous with achieving a chosen position; but again the advantages of this construction are not exploited.

because of the penumbra of vague and contentious metaphysics that surrounds them.

3. It is sometimes complained that, according to the nominalist view of definitions, we might just as well attach the word misery instead of welfare to our index, or the word stupidity to a positive index of intelligence—that is, that the choice of terms is arbitrary. What is forgotten is that, if these terms are introduced only as a shorthand, for convenience in expression, they should be conveniently chosen, which rules out perverse usage.

4. Little presents several arguments, some of which we have already considered. His main argument, however, appears to be that all welfare propositions are framed in emotive language, and so have an ethical content.[1] It is a good point that the language in which welfare propositions are framed is often of a persuasive character which obscures the explicit value judgments that are necessary for prescription. But it does not follow from the emotive nature of language, or of language carelessly employed, that the judgment " increasing welfare is good " is a necessary premise to the proposition " this is how we could increase it ". Little's argument, however, is not only a *non sequitur;* it would mean, if it were correct, that there could be no such thing as positive economics. If " full employment " or " inflation " have emotive connotations, then, according to Little's view, there could be no non-ethical enquiry into the causes of the phenomena to which these phrases refer. His argument, indeed, would leave us without any positive disciplines at all. Consider the emotive implications of physics to one who believes that the manufacture of atom bombs is immoral. It is as well that the argument is a *non sequitur:* otherwise we should have nothing but metaphysics and logic.

Little's position, however, does seem to be that there is nothing but normative metaphysics and logic. He writes:[2]

> " Welfare economics and ethics cannot, then, be separated. They are inseparable because the welfare terminology is a value terminology. It may be suggested that welfare economics could be purged by the strict use of a technical terminology, which, in ordinary speech, had no value implications. The answer is that it could be, but it would no longer be welfare economics. *It would then consist of an uninterpreted system of logical deductions, which would not be about anything at all*, let alone welfare. *As soon as such a system was held to be about anything*, for example, welfare or happiness, *it would once again be emotive and ethical.* Getting rid of value judgments would be throwing the baby away with the bathwater. The subject is one about which nothing interesting can be said without value judgments, for the reason that we take a moral interest in welfare and happiness."

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[1] *Critique*, chap. V.

[2] *Ibid.*, pp. 81–2. Italics mine.

The *non sequitur* in the second sentence has already been pointed out. The last sentence also confirms what has already been argued, that, according to Little, there can be no positive, scientific enquiry into the causes of full employment or inflation, bad weather or atomic explosions, in all of which we may take a " moral " interest. But consider now the passages italicised. Little has implicitly set up two exhaustive and mutually exclusive classes between which all propositions must be divided. One class contains " uninterpreted logical deductions about nothing at all ", and the other " interpreted " deductions of an ethical nature. Little admits no other class of proposition to his universe. All is uninterpreted logic—or metaphysics. It is more convenient, as well as more usual, to allow a class of " interpreted " propositions about which we ask the question: are they refutable ? In Little's classification this question is irrelevant.

Graaff, we may notice, offers the argument about terminology in a very much weakened form. After remarking that economists might " . . . learn to regard the ' national income ' as no more than ' that which this index measures ' . . . " he rejects the nominalist view on the grounds that " . . . it is too much to hope that laymen will do likewise . . . ", and argues that, since a term like " national income " acquires emotive connotations, the question " has it increased ? " is normative.[1] The danger that laymen will not accept the nominalist position is hardly adequate grounds for refusing to do so ourselves. It seems that there is some confusion between two separate questions, the domain to which welfare economics belongs, and the language in which the results of economics are best communicated to the public. If we try to settle the former question by reference to the latter, we shall indeed get in a muddle.

5. On a different issue, both Little and Graaff argue that welfare economics and positive economics are distinguished by the fact that the conclusions of the former are not testable. In reply to this, there are several points to make.

(i) Is this actually true ? If the word " welfare " appears in the conclusions, and if the word is understood, not as a shorthand for an index, but as a formalization of an intuitive concept, it doubtless is true. In this case, the whole subject of welfare economics belongs to metaphysics. But is a theorem of the form " this change expands the individual's choice " irrefutable ? It seems that there is a confusion between the testability of the metaphysical interpretation of the welfare proposition, and the proposition itself. The following propositions, some of the most interesting in welfare economics, could all be stated in such a form that they were refutable :[1]

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[1] Graaff, *op. cit.*, p. 2. I must make clear why I have singled out Little and Graaff, among all the authors on welfare economics, for criticism in this paper. Little I have chosen precisely because he treats at some length the matters with which I am concerned, and sets out explicitly a view which I believe is quite mistaken. Graaff, on the other hand, provides an excellent example of how easily the muddle may be cleared up. As we have already remarked (p. 320, n. 1), if a few passages are deleted from his book nothing of substance is lost, and a source of serious misunderstanding is removed. Consider, for example, his statement, already referred to, that, before we work out the necessary conditions for an economy to be on its production frontier, we must make the value judgment that it is better to be on it than inside it. This judgment is quite irrelevant to the analysis, and the sentence may be struck out without in any way damaging Graaff's argument.

(*a*) all propositions of the sort: with this change, the individual moves to a chosen position (the possible refutation is in two parts: one, the budget line may not move as predicted; two, the individual may not choose as predicted, i.e., substitution effects may be negative);

(*b*) the " technological " propositions, such as: if the marginal productivity of a factor is greater in one place in which a certain good is produced than in another, output will increase if units of the factor are taken from the former place and added to the latter (in welfare economics we are usually concerned with suitable generalizations of such propositions which, at the micro-level at least, are obviously refutable);

(*c*) the familiar Paretean propositions, such as: if a perfect market is in equilibrium, there is no change which permits an expansion in the choice of any consumer(s) without contracting the choice of at least one other consumer ;[2]

(*d*) the propositions of international trade theory, and particularly the most important, " trade expands choice ".[3]

This incomplete list should be sufficient to make the point: the technological theorems of welfare economics can obviously be made refutable; the others are refutable if they are stated in terms of a refutable demand theory. If they are stated in terms of non-refutable demand theory, they are clearly non-refutable.

(ii) Suppose that a proposition in welfare economics is irrefutable. Does this of itself make it a normative proposition ? Little, and even Graaff, seem to think that it does. But for this to be the case, propositions must have been implicitly divided into two exhaustive and mutually exclusive classes; the refutable and the normative. This classification, however, is not proposed; and it would be exceedingly inconvenient. Consider the proposition " there are sea-serpents ".[4] It is irrefutable because, no matter how long our search is unsuccessful, it is always possible that, if we look a little longer, we shall find a sea-serpent. It is not a scientific proposition; and we may call the class of propositions excluded from science on the grounds of their irrefutability metaphysics. But is this proposition normative? If I go further, and assert " not only are there sea-serpents, but they like to eat babies ", am I making value judgments? It is far more convenient to subdivide metaphysical propositions into the normative and the positive. We should notice, incidentally, that the practical difficulty of testing a proposition cannot turn it from science to metaphysics. Thus the proposition that " there are no sea-serpents " is refutable (by the discovery of just one single specimen), no matter what the difficulties of organizing a thorough search of those parts of the depths of the ocean in which sea-serpents might reside. It is very likely that we shall have more interest in propositions which it appears feasible to expose to the risk of refutation; but the difficulty of exposing them does not turn them into normative propositions if they are of such form that they *could* be refuted.

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[1] I have endeavoured here to state them in testable form. As commonly stated, they are untestable. If, for example, marginal productivity is defined as the *actual* addition to output of an extra man, (*b*) is tautological. To make it testable we require an *ex ante* definition of marginal productivity. We might, for example, extrapolate from observed points on a production function. The prediction (*b*), suitably formulated, might then be refuted if, for example, the transfer aroused the suspicion or hostility of workers who worked less hard in consequence.

[2] Cf. K. Lancaster, " Welfare Propositions in Terms of Consistency and Expanded Choice ", *loc. cit.*

[3] This is refutable in the same way as propositions (*a*) above.

[4] For this illustration, as for much else, I am indebted to the writings of Professor Popper.

(iii) Graaff and Little appear to have thought that, the conclusions of welfare economics being supposedly non-testable, we should test the assumptions, " factual and ethical ". Consider first the testing of ethical assumptions. As has already been pointed out, their testing necessarily involves appeal to our intuitions. " Testing " now has a meaning quite different from its meaning in science: it involves such questions as " do we want it? " or " does it correspond to our ideal forms? " It is seriously misleading to speak of " testing " ethical assumptions and testing factual ones as though the two operations were identical. Consider now the testing of factual assumptions. Whether or not this operation is possible, or, if possible, establishes anything about the conclusion, is a question of the methodology of positive economics.[1] Notice, first, that assumptions of utility, or profit maximization, can only be tested by comparison of the conclusions drawn from them with the results of observation; the assumption of a stable propensity to consume can be directly tested. Even when an assumption has been tested, however, this establishes nothing whatever about the conclusions of the theory. In the first place, the testability of the assumption is not even a guarantee that the conclusion has content. In the second place, a model may easily include well tested assumptions but, because the wrong functional relations have been assumed,[2] or because relevant variables have been omitted, it may be refuted if it is exposed to test. Investigating assumptions, when possible, is a useful activity, but it can never assure us that we are going to reach correct conclusions; and if the conclusions are not even testable, we can never find out if they are wrong. Accepting non-testable conclusions is a simple act of faith; if the conclusions of welfare economics were non-testable, the whole subject would be metaphysics.

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[1] On which see Milton Friedman, " The Methodology of Positive Economics ", in his *Essays in Positive Economics*, 1953. Little, it will be recalled, has asserted that there can be no methodology of welfare economics.

[2] It should by now be evident that " assumptions " in economic theory may be of so many sorts that it is unwise to speak about testing them as though they were a homogeneous class.

* * * *

This concludes my account of the objections to the nominalist view with which I am acquainted. The conclusion is easily written. There is a single dichotomy in economics, between positive and normative. So long as we are enquiring into the relationship between ends and scarce means, our enquiry is positive. Ends are not divided into two categories, in such fashion that we must, if they belong to one category, preface our enquiry with a judgment upon them, while, if they belong to the other, we need not. Our private judgments about ends do not of themselves endow our enquiry with ethical implications. Nor does the emotive nature of language of itself give the relationships we discover between means and given ends an ethical significance, although it is as well to be warned against the careless use of persuasive terms.

There is one last consideration: caution. Little intended not only to warn us against persuasive language, but also to expose all the value judgments that must be made if welfare economics is to have prescriptive force. Yet if the distinction between positive and normative is not recognized (Little argues that *all* systems which are " about anything " are ethical), caution cannot be easy. And if we start from our notion of what is good, enthusiasm is always likely to be the enemy of caution. If we believe, with Mishan, that welfare economics is based upon a " cautious ethic " which is widely acceptable,[1] we are likely to forget due caution when we turn from welfare theory to prescription. It is much easier to be cautious, and to remember the many considerations on which policy prescription must be based, if we treat our welfare theory as positive, and insist that *all* the value judgments come when the crucial step to prescription is taken. It is this practical question, of how we proceed from welfare theory to prescription, that gives importance to a methodological argument of the sort conducted here. It is also of some importance that we might greatly extend the interest and applicability of welfare economics if we endeavoured to formulate our propositions in testable form.

The London School of Economics.

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[1] Mishan, " Some Alleged Contradictions ", *loc. cit.*, p. 452.

Underemployment in Agriculture and the Industrial Wage Rate[1]

By Dipak Mazumdar

Students of the underdeveloped economies have to explain why the earnings of labour are often substantially higher in industry than in agriculture even after allowance is made for the higher cost of living in the towns; and this despite the absence of any strong trade union organisation and the presence of a large and mobile surplus of labour both in agriculture and in the urban districts around the factories. One answer has been given by Messrs. Bauer and Yamey, that, " in subsistence economies the extended family system (or analogous arrangements) provides for a form of collective . . . production and consumption . . . which in effect establishes a reserve price for [the unemployed person's] labour ".[2] This hypothesis, while it shows how there will be a floor to the wage rate given by the income *per capita* in agriculture, does not show how the earnings of industrial labour can be substantially higher than this level. In fact, in many underdeveloped countries earnings of labour in industry are not merely higher than the *per capita* income in agriculture (i.e., average income for all members of a family—employed, underemployed and unemployed) but also than what one would expect to be the transfer price of labour under conditions of full employment—viz., the income *per earner*. Nor does the above hypothesis explain how earnings—even of unskilled labour—in factories are higher than those of the mass of low-wage labour found in occupations other than factory industry in the same urban area.

The purpose of this article is to draw attention to a factor which has not been noticed in the literature, but which may be quantitatively of great importance in some economies in setting wages in industry at a level higher than in agriculture. It is essentially connected with the fact that the wage rate sufficient to induce families to move from agriculture to industry is substantially higher than that needed to attract individual members of rural families. Certain conclusions about the structure of the labour market in underdeveloped countries and about the hypothesis of the perfectly elastic supply curve of labour to non-agricultural activities can be derived by way of corollary from the existence of the different types of migrant labour with different supply prices.

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[1] I am grateful to Professor E. H. Phelps Brown and Mr. B. S. Yamey for reading earlier drafts of this paper and suggesting many improvements. My thanks are also due to Mr. A. D. Knox for valuable comments.

[2] P. T. Bauer and B. S. Yamey, *The Economics of Under-developed Countries*, 1957, p. 77.

I

We shall first consider the supply price of migrants from agriculture. We abstract from all non-pecuniary advantages or disadvantages attaching to different employments. We also ignore costs of movement and differences in rural and urban cost-of-living by considering (implicitly) earnings, wage rates, etc., in real terms.

We assume an over-supply of labour (i.e., unemployment or underemployment) in the rural areas, and the competitive determination of wages both in town and country. A question arises here as to the determinants, under conditions of over-supply of labour, of the level of earnings of labour in agriculture. But since this is not the problem we are considering in this paper,[1] we shall simply take as given the actual incomes of both cultivators' and labourers' families (and the actual wage rates).

Migration from the agricultural sector can take various forms, and the supply price of the migrants will vary with the type of migration. It can, however, be seen that if we assume (i) full employment of labour in agriculture and (ii) identical employment opportunities for the members of a family in town and country, there will not be any difference in the supply price of individual and family migrants: the loss of income due to the departure of the migrant in either form will be equal to the income *per earner*, and this will give the supply-price. It is the prevalence of disguised unemployment and the difference in employment opportunities in rural and urban labour markets which make it necessary to distinguish three migrant types with different supply-prices:

(*A*) Individual migrants from land-occupying families;
(*B*) Individual migrants from rural wage-paid labourers' families;
(*C*) Family migrants from both the land-occupying and the labouring classes.

We deal with each type of migrant in turn.

(*A*) An individual migrant is defined as a person who leaves his family in the country and seeks urban employment for long or short periods. In this case the earning strength of the family is split up between the country and the town.

" Disguised unemployment " is characterised by the unremunerative employment of some members of the labour force, whose earnings are much less than the average. In particular situations this may take the form of the earnings of *all* earners of *some* families falling below the average earnings in the occupation concerned. (This is probably the case in cyclical underemployment to which Mrs. Robinson first applied the term " disguised unemployment ".)[2] But where disguised unemployment is chronic, as on family farms in some densely populated underdeveloped countries, unremunerative employment will affect *some* earners of nearly *all* families, at least of those in the income zone from which migration usually takes place. In this case, the withdrawal of one earner from the family labour force will lead to a loss of income much less than the average income *per earner*, because his labour can easily be replaced by the labour of other members. What will be the transfer price of an individual migrant in this case? Three possibilities emerge:

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[1] On this problem, see H. Leibenstein, " The Theory of Underemployment in Backward Economies ", *Journal of Political Economy*, April, 1957; also my forthcoming article on " Marginal Productivity Theory of Wages and Disguised Unemployment " in the *Review of Economic Studies*.

[2] Joan Robinson, *Essays in the Theory of Employment*, 1937, pp. 60 ff.

(*a*) If the marginal contribution of an earner to the family income[1] is less than the income *per earner*, but greater than the income *per capita*, the transfer price will be the marginal contribution.

(*b*) If, however, it is *less* than the income *per capita* of the family, there are two possibilities: (i) If the aim is the maximisation of individual income (without reducing the income of the family), then the transfer price will be the income *per capita* of the family. For while the individual gains by having an income greater than the *per capita* income, the family gains by the departure of the migrant himself, since his contribution to the family economy *qua* producer is smaller than his " drain " *qua* consumer. (ii) When, however, the migrant is interested in the maximisation of the family income without any concern for his personal income during the period of absence in the town, the supply price will be given by the marginal contribution to the family income; the migrant loses personally by going to town, but the family secures a net gain. The proviso must be made, however, that when the marginal contribution of a migrant is very low (in the limiting case, zero), a lower limit to the supply price is set by the cost of subsistence in town, or, if we admit the possibility of a flow of funds from country to town, by the difference between the costs of subsistence in town and country.[2] Even the higher of these two lower limits will, however, be less than the *per capita* income of rural families, except for marginal families.

If the marginal contribution of workers in a large number of family farms is small, and the migrants are not unduly family-conscious, it is reasonable to suppose that migrants of this category will be predominantly available at the *per capita* income.

(*B*) When we come to wage-paid labour in agriculture, disguised unemployment takes a rather different form from that in cultivating families. For agricultural labourers it is more correct to speak of underemployment rather than disguised unemployment.

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[1] This phrase helps us to take account of the fact that in farming the earnings of any one earner will not be directly known, but the loss of earnings due to the departure of any earner can be attributed to him.

[2] The possibility of such a flow of funds is limited, for, among other things, the income accruing to an agricultural household is largely in kind, and the surplus cash income may be too small for the use of the migrant in the urban money sector. But the flow of funds may and does take place, to a limited extent, in an indirect way. This is when a new migrant to the town is partly maintained by his relative already employed in town out of income that would probably have been otherwise remitted to his village household.

At the going wage-rate there *is* an excess of supply of labour as shown by the fact that the average worker secures employment for only a limited number of days in the year. But the excess supply is found not as the permanent idleness of some, but rather as the less-than-full employment of all members. The system of hiring labour is on a day-to-day casual basis, and under chance selection in which everyone has an equal chance of employment, the available volume of employment in the long run comes to be shared out among the total working force, and the earnings of a worker during the year are less than they would have been if he was fully employed. If there is complete chance selection then, in the long run, all workers would have the same frequency of employment. But even if actual conditions do not represent a case of complete chance selection, the days of employment secured by most workers, and hence their earnings, will not deviate far from the average employment and earnings per worker. Thus in this case there will not be a large difference between the marginal contribution of a worker to the family income and the average income *per earner*. The loss of income, equal to the average earnings, will be the same whether we consider the individual or the family, because, when the volume of migration is relatively small, the migrants' departure will not increase significantly the average employment (and hence income) of the remaining members. Consequently, as in the case of full employment of all regular earners, the supply price of migrants will be given by the income *per earner*, which, for the same level of family income, is higher than the supply price of category (*A*).

The number of migrants available with a supply price below the average earnings of rural labourers will obviously depend on the number of land-occupying families with an income level much the same as that of labourers' families. This is, indeed, likely to be a numerous group in over-populated countries, where large numbers of small cultivators occupy the same income range as landless labourers. Where the equalisation of incomes of the various agricultural classes has gone so far, it may be asked what prevents the " redundant " members of cultivators' families from seeking employment as wage-paid labourers in agriculture. Members of cultivators' families do, indeed, seek work as rural labourers, but the number of days of work secured by them is usually much less than that secured by members of the labourers' families. Institutional arrangements protect the total employment available for labourers' families (in many parts of India the agricultural labourers are a definite caste), and the daily wage rate, as already mentioned, is high enough to yield the labourers' families the conventional income, given the number of days of work they succeed in securing on the average during the year. Thus, even after allowing for the possibility of work as rural labourers, the marginal contribution of a member of a farm-owning family remains lower than the average earnings in rural employment.

(*C*). *Family Migrants*. We may now turn to the supply price of migrants who settle in town with their families. Here the source of income in the village is totally lost. Provided that the earning opportunities open to the various members of the family are the same in the town as in the country, the supply price of a member of a family will be determined by the income *per earner* in the country, and in this respect it is immaterial whether the family is a cultivating or a labouring family. For members of agricultural labouring families, the supply-price of individual migrants is likewise the income *per earner*, so that for this class there is no difference between the supply price of individual or family migrants. But for cultivators, because of the small marginal contribution of a worker to the income of the farm family, the supply price of an individual migrant is substantially below the income *per earner*, and hence below the supply price for a migrating family. It should be noted that, even apart from round-the-year underemployment on farms, in so far as lone migrants from farms can be absent temporarily during slack seasons or for a short period of their working life without impairing the productivity of the farm, the supply curve of individual migrants below the level of the income *per earner* (and hence below the supply price of family migrants) is extended.[1]

But suppose that permanent family migration takes place entirely from among families of agricultural labourers, or that the surplus labour on family farms has been drained off so that the supply price even of lone migrants has risen to the level of the income *per earner* of cultivating households, will the supply price of individual and family migrants coincide ?

They will coincide only on one assumption—and here we come to a very important point about labour market conditions in underdeveloped countries—viz., that the earner-dependent ratio is the same in the town as in the villages. Only when this is so will the same income per earner yield the same family income in town and country. If proportionately more members of a family can be " effective " earners in the town, the family income will be larger; if fewer, the family income will be smaller. What we find, in fact, is that sometimes the earner-dependent ratio is actually lower in the towns, primarily because the women who are part of the labour force in agriculture are rarely engaged in employment in factory industry. In so far as this is so, the " effective " supply price for urban employment has to be higher than the average earnings in the rural areas by an amount sufficient to compensate for the " adverse " earner-dependent ratio. Thus the supply price of family migrants will be entirely above that of individual migrants for all volumes of migration.

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[1] If the agricultural labourers are seasonally idle for a reasonable stretch of time they could be available at a supply price below the income *per earner* during these periods; but if the seasonal variations in wage employment are not significant, labourers will lose their wage income whether they are absent for short or long periods, and hence their supply price would still be the income *per earner*.

There are two other, more obvious, reasons why this should be so. First, the difference in the cost of living between town and country will be higher in the towns for family migrants than for individual migrants (house rent being an important part of the higher cost of living in towns). Secondly, the family will need a greater cushion against misfortune than the individual.

II

We thus have three segments of the supply curve of migrants from the rural areas: first, the underemployed lone migrants from land-occupying families with small marginal contributions to the family income (available predominantly at the *per capita* income); second, the individual migrants from labourers' families with a supply price given by the income *per earner*; third, the family migrants from both classes whose supply price is *higher* than the income *per earner* because of the lower earner-dependent ratio in town. We shall trace the implication of these segments of the supply price for the determination of the wage rate in urban industry.

If the three types of labour whose supply prices are different are homogeneous from the point of view of the demand for labour in industry, the industrial wage will depend on the relative numbers of the three groups of migrants and the total demand for labour. If the demand for labour exceeds the supply of the first two groups, the wage will be determined by the supply price of family migrants. In fact, however, this is not the situation in countries with an over-supply of labour in agriculture, and in which the volume of migrants available to an urban labour market from the surrounding rural areas is large compared with the volume of employment in urban industry.[1] One would expect, then, that the supply price of family migrants would not be effective in the determination of urban wage rates.

There is, however, an important factor on the demand side for labour which ensures that family migrants with a higher supply price (and, hence, wage) are employed in large-scale factories in some urban industries even when individual migrants are available at a lower wage. This is connected with the fact that family migrants provide the bulk of a stabilised labour force for industry, and that in some industries there is an important direct relationship between the stability of the labour force and its efficiency.

The financial cost of labour turnover has been recognised in advanced industrial countries. Where, as in some underdeveloped countries, a large settled urban working-class does not exist, these costs of labour turnover may be magnified. This is because adaptation to factory technique and factory discipline is a new experience for the migrant seeking work in industry. The appropriate attitudes and new behaviour-pattern are slowly acquired. Further, the responses of individuals to the new discipline are likely to vary widely. Thus the gap between the productivity of a new recruit and the replaced worker may be substantially greater in a non-industrial country than in an industrial one.[1] Consequently a factory with a more or less permanently attached body of labour can attain a higher level of productivity than one with a high labour turnover. Even if a trained migrant returns to work regularly after an interval in the country, he does not retain his skill[2]. Thus, in a Durban survey it was found that while the " reasonably good Native could attain a level of productivity of about 81 per cent. of the European worker, the productivity of migratory hand-picked Natives came to 29 per cent."[3] Apart from these direct short-run gains from labour stability, it has been noticed that the change in attitude to work that comes from a settled labour force is an important factor in augmenting productivity in the long run.[4]

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[1] See below, p. 336.

The lower earner-dependent ratio in the town, in fact, ensures that lone migrants will always be available at a supply-price below that of family migrants; so that if there is a limit to the volume of lone migrants it will be given by social factors relating to the refusal of migrants to live apart from their families. Because of the low income of the rural families, this limit may come only after a large volume of migration. (The existence of disguised unemployment on farms, as has been examined in section I, widens the gap between the supply prices of individuals and families.)

The higher efficiency of stable workers means that it is profitable for some industries to have a stabilised labour force at a higher wage, even when other workers are available at a lower supply price. If, then, family migrants provide the stable labour force, the industrial wage rate will be determined by the supply price of family migrants; while the non-stable migrants will not affect wages in the market for a stable labour force. It should be noted that it is not necessary for this conclusion that the distinction between family migrants and individual migrants should coincide exactly with that between stable long-term and irregular short-term workers. Some individual migrants may, in fact, be long-term. But as long as the demand for long-term labour exceeds the supply of individual migrants willing to form part of a stable labour force, at a wage less than the family migrants' supply-price, the latter supply price will govern the industrial wage rate.[5]

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[1] The fact that the overall skill to be attained is often less for an industrial worker in the non-industrial countries is not a wholly compensatory factor. The major hurdle we are considering is that of the new recruit getting used to the rudiments of factory discipline and skill, not that of passing between different levels of efficiency.

[2] Cf. *The African Factory Worker* 1950, ch. v (Natal University: Natal Regional Survey. Additional Reports. No. 5).

[3] *Ibid.*, p. 97.

[4] Cf. P. de Bricy: "Productivity of African Workers", *International Labour Review*, 1955; Boris Grossman: "Industrial Efficiency and the Urban African", *Africa*, 1953.

[5] It does not follow that individual migrants alone will be employed in the stable labour force even if there are enough of them who are willing or anxious to form part of such a labour force. First, employers may have doubts about the occupational stability of lone migrants and hence be unwilling to rely on them to any great extent. Second, the stability of some of the individual migrants may itself depend on the prior existence of families settled in the towns. (Many individuals are found to reside with their relations who are already settled in the working-class districts in Indian cities.)

The family migrants will form the industrial work-force, and their (higher) supply price will determine the industrial wage rate, so long as the ratio of the productivity of stable to that of non-stable labour exceeds the ratio of their supply prices. In fact, however, the ratio of the productivities of the two types of labour is not likely to be independent of the proportion of the two in the total working force, since a non-stable worker will be substitutable for a stable worker up to a certain point.[1] It seems more likely that a nucleus of stable labour is required for the organisation of a factory on a high productivity basis, and that, as non-stable workers are added to this " core ", a dilution of the labour force takes place with a reduction of efficiency. The " returns " to the use of non-stable labour probably diminish at a slow rate at first, but then at a more rapid rate as the increasing labour turnover progressively impairs the organisation of the factory and the planning of operations. If there is this relationship, will our conclusion about the industrial wage rate being determined solely by the supply price of family migrants be valid ?

The analysis given above and the conclusion reached so far will be unchanged if we make two assumptions: (i) there is no wage discrimination possible between the two types of labour with the two supply prices; and (ii) the supply of stable labour (family migrants) is perfectly elastic. In this case, at a certain wage, the entire work force will be made up of stable labour: below this wage no stable workers will be forthcoming. Thus, in spite of the fact that some non-stable workers may be substitutable for the stable workers at a lower supply price, no non-stable workers will be employed.

But if there is some wage discrimination, then a certain amount of non-stable labour could be combined with a given amount of stable labour, i.e., up to the point when the lower efficiency of the marginal non-stable worker employed just offsets the lower wage cost of employing him. There will then be two wage rates in industry, and the composite wage rate will be below the supply price of family migrants by an amount depending on the extent of wage discrimination and the proportion of non-stable workers that it is profitable to use in the total work force.

Moreover, even when it is not possible to have wage discrimination between the two groups of workers, it is still profitable to use some non-stable workers along with a given volume of stable workers if the supply of the latter is not perfectly elastic; for by using non-stable workers, the required wage level for the stable workers will be lower than otherwise. For any amount of stable labour, an amount of non-stable labour can be profitably added to it, the size of the non-stable component being given at the point where the marginal cost of employing an additional non-stable worker (influenced by the lower efficiency of the marginal unit)[1] equals the marginal cost of employing an additional stable worker (influenced by the elasticity of the supply of stable migrants).[2]

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[1] This is so especially where the difference in productivities of the two types of labour is due not so much to the difference in their inherent capacities as to the higher organisational costs which the non-stable workers with their higher turnover impose on the firm.

The conclusion of the above analysis is that in certain situations the wage rate in some urban industries will be determined by the supply price of family migrants who provide the nucleus of the stable labour force, even though the partial utilisation of non-stable labour may lead to a wage rate lower than otherwise. Moreover, and more generally, the analysis provides an explanation of the phenomenon that industrial wages are maintained, even in the absence of strong trade unions, despite the fact that there is a large supply of labour in urban labour markets adjacent to the factories with earnings substantially below those secured by factory workers. A plethora of low-earning labour is found in urban services, casual labour and petty trade.[3] The differential in wages between agriculture and industry cannot thus be explained in terms of labour immobility. But this discrepancy as well as the apparent inconsistency in the urban labour market can be explained in terms of differences in the supply of, and demand for, different segments of the flow of labour from country to town. Many of the workers in miscellaneous urban occupations are individual migrants with low supply prices. The presence of these low-income groups of workers, who are such a conspicuous phenomenon of urban areas in underdeveloped countries, is evidence of widespread underemployment in agriculture which keeps the supply price of individual migrants at a low level; but it is quite compatible with the higher equilibrium wages determined by the supply price of family migrants in the factories.[4]

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[1] The supply curve of stable (family) migrants being entirely above that of non-stable migrants, an unlimited supply (for practical purposes) of non-stable migrants will be available at every wage needed to attract a certain volume of stable labour.

[2] In the textile industries in India there has been a system of labour utilisation in which, along with a stable core of workers with a long period of service in the mills, there is a partial utilisation of casual, non-stable labourers called *badlis*, employed on a day-to-day basis. The non-stable segment seems to have been about 25 per cent. of the total. This system is described in detail in my doctoral thesis submitted to Cambridge University. It provides the basis for the set of considerations given in general terms here. Wage discrimination in the Indian mills between the two wings of the labour force was limited in extent; so the employment of the non-stable workers has to be attributed largely to the shortage of stable labour at the going wage-level, i.e., to the inelasticity of supply of stable labour.

[3] The extent of such employment can be gauged from the fact that for Calcutta City in the " inferior " occupations (i.e., offering employment to workers of roughly the same socio-economic strata as that of factory workers), services, trade and miscellaneous occupations accounted for about 610,000 earners as against 250,000 in industry (of which half was in small-scale industry). Small trade and hawkers alone accounted for 253,000. (Data from *Census*, 1951, and the *Report on the Unemployment Survey*.)

A report on wages of shop assistants in Bombay City (which resembles the labour market of Calcutta in its broad structure) in 1935, showed that 32 per cent. of the shop workers earned less than, or as much as, the unskilled factory workers. (Data from *Report on Wages in the Retail Trade*, Labour Office, Bombay, 1936.)

[4] The contrast, as regards the permanency of migration, between the labour attracted to miscellaneous urban occupations and to permanent factory employment, is revealed by the following data for Bombay City. According to the Census of

III

When it is realised that it is the income of the farm family that sets the level of the equilibrium wage in certain industrial employments, it is easy to see that money wages in industry can be maintained at an apparently high level while labour in the agricultural sector is surplus. Labour in agriculture may be surplus in two senses: first, in the sense that when some of it is withdrawn, production can be maintained without any change in technique or supply of co-operant factors; and second, in the sense that actual idleness of labour engaged in agriculture, as a percentage of total man-days available, is substantial. The phenomenon of underemployment, expressed in either or both of these two ways, may be widespread in the agricultural sectors of some economies. But this does not mean that the farm families, among whom land is distributed, do not enjoy a positive income. It is this income level which sets the supply price of labour.

This proposition calls for a reconsideration of models of economic growth based on the hypothesis of a perfectly elastic supply curve of labour, such as that expounded by Professor W. A. Lewis.[1] This model envisages a sustained rise of the profit (and hence savings) ratio in the capitalist sector with the expansion of employment in this sector, because the additional equipment per worker created by the additional workers employed continually shifts the productivity curve upwards, while the real wage remains constant. The workability of this model depends on either of two assumptions: (i) the supply curve of labour is perfectly elastic for a considerable stretch of employment of workers in the capitalist sector; and (ii) a large gap exists between the absolute wage level and the value product of labour in the capitalist sector, in which case some rise in the real wage due to additional employment would not lead to a squeezing of the profit ratio; or, to look at it in a different way, the wage rate could be set at a point which is high enough for labour to be in unlimited supply, but which still leaves a large enough profit-ratio for capital investment per worker to increase continuously. The conclusions reached above would seem to cast doubt on the validity of both these assumptions for some parts of the underdeveloped world.

First, it is tempting to assume a very low value for the wage-product ratio in underdeveloped countries (compared with industrialised countries) because the cost of subsistence at which unemployed labour is expected to be available for urban industry is so much lower than the value product per worker in industry. Even when the supply price of labour to industry is recognised to be the *per capita* income in

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1951, there were only 384 females to 1,000 males in Bombay City, which reflects the extent of lone migrants participating in its labour force. In contrast to this, in spite of the fact that the permanent factory workers were practically all immigrant, 80 per cent. of them were settled in the City with their families. (*Family Budget Enquiry in Bombay City*, Government of India, 1944.)

[1] " Economic Development with Unlimited Supply of Labour ", *Manchester School*, June 1954.

agriculture, it may still seem that the wage-product ratio will be lower in underdeveloped countries because the difference in the *per capita* income between the underdeveloped and advanced countries is significantly larger than the difference in the value product per worker employed in industry. However, the false optimism in this conclusion arises from the concentration of attention on the movement from country to town of individuals rather than of families, which leads to a serious underestimation of the relevant supply price of labour to industry. Whether the migrant goes to town as an isolated individual or with his family, he may be enjoying the same real income as in the rural areas; but the wage rate that will need to be paid to him to yield this income will be widely different in the two cases.[1] Thus, when the industrial wage rate is determined by the supply price of family migrants, the wage-product ratio will not be so low as might be expected when attention is focussed only on the large-scale underemployment in agriculture.[2]

Second, the ready acceptance of the hypothesis of a perfectly elastic supply curve of labour is also due to the failure to distinguish between the two different types of labour attracted to the urban areas—the stable family migrants needed for some types of large-scale industry and the shorter-period non-stable migrants. As we have indicated, individual members of agricultural families may be unemployed or underemployed, but there is no such thing as unemployment of entire families (except in the case of violent changes in the system of land tenure). There are several important reasons why the supply-curve of the unemployed or underemployed individuals should be highly elastic, but that of families less so. To begin with, agricultural families have different income-levels, and this fact, in itself, will make their supply-curve inelastic. The supply price of underemployed individuals, so long as they are interested in maximizing family income, will be the cost of subsistence in town whatever the level of income of the family they come from (their departure is a net gain for the family if their marginal contribution is less than the *per capita* income).[3] If the individual migrant wants to maximise personal income, his supply price will vary with the *per capita* income of the rural families, but, even in this case, the supply of individuals will be more elastic than that of families. They will have greater geographical mobility; they will be interested in the earning opportunities in the short run without so much regard for the security of the income stream, so that they will be much more sensitive to economic " pulls " and " pushes "; lastly, family migrants to the same urban area are faced with higher costs of housing, which is a much less important factor for individual short-period migrants.

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[1] Where, as in India, the average family size is four or five, and there is usually only one earner in the family in the town, the wage rate for the family migrant would need to be four or five times higher than that for the individual for the same real income per head.

[2] Thus, as early as 1927, when labour was not organised at all, the Tariff Board found that the main reason for the depression of the textile industry in Bombay was the high labour costs. (Indian Tariff Board, *Report of the Cotton Textile Industry Enquiry*, 1927, vo. I, chs. VII–IX).

[3] See above, section I.

Whether the supply of non-stable labour at low wages is infinitely elastic or not, it may be tempting to conclude from observation of the urban labour market that labour will be in unlimited supply at the current wage, since it contains large groups of workers whose earnings (and hence supply-price) are so obviously below current factory wages, and they show such a high degree of mobility.[1] But these are, as already indicated, largely labour of the non-stable type. The existence of a substantial pool of non stable labour in the urban areas is consistent with a shortage of labour for industrial expansion. The supply of family migrants to constitute a stable industrial labour force will have a higher supply price; moreover, its supply is likely to be inelastic even at this high wage. Thus, starting from a wage-product ratio that is already high, there may not be much scope for the indefinite expansion of employment at constant low wages and with growing profits as envisaged in Professor Lewis's model.[2]

IV

In conclusion, it should be noted that what has been said about the determinants of wages in large-scale factory industries need not be valid for all types of organised industry. Our analysis of the varying levels of supply-price for different types of migrant will already have suggested that the wage rate in any labour market is determined by the characteristics of labour needed in the particular activity. The two important variables here are, first, the stability of the work force required, that is to say, the relationship between stability and productivity in the activity; and second, the opportunity for employment of subsidiary earners (especially women) in the families attracted to the activity. Thus seasonal industries which can attain reasonable efficiency with floating labour need to have a wage level sufficient to attract unstable individual migrants only; and their wage rates are substantially below those in permanent factory industries.[3] Similarly, plantations do not require their labour force to be as stable as the mills, and they may in addition offer opportunities for employment of women; wage rates on plantations are thus also lower.[1]

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[1] Mr. P. T. Bauer has drawn attention to the fact of the substantial mobility of labour supplied to Malaya and Ceylon, even in the caste-bound backward parts of South India (in his *Economic Analysis and Policy in Underdeveloped Countries*, 1957, p. 19). The extensive supply of labour to the urban areas—rising and falling with booms and depressions—is one of the most conspicuous phenomena of underdeveloped countries.

[2] Prof. Lewis writes: " When we take account of all the sources we have now listed —the farmers, the casuals, the petty traders, the retainers (domestic and commercial), women in the household, and population growth—it is clear enough that there can be in an over-populated economy an enormous expansion of new industries or new employment opportunities, without any shortage of unskilled labour being apparent in the labour market " (*loc. cit.*, p. 145). It is interesting to note that four of the six sources listed—the casuals, the petty traders, the retainers and women in the household—are precisely the groups which are likely to be unavailable for the stable labour supply at their current level of earnings.

[3] In the seasonal cotton ginning factories in urban areas of Bombay province, the majority of men were paid 5–7 as. per day in 1934, when the wages of unskilled

Wages even in a large-scale factory industry may be relatively low if special systems of labour utilisation can tap the types of migrants which have low supply prices. The classic example is the employment of girls in the Japanese textile industry. In 1933, " slightly more than three-quarters of the total number of workers employed were . . . girls under 25 years of age ".[2] They were, moreover, employed for short periods before their marriage.[3] They could thus be recruited at very low wages, with the result that the average wages in the textile industry for all operatives came to no more than 40 per cent. of the general average in all industries.[4] At the same time, the system of recruitment and boarding of girls in the textile factories made for stability of the labour force and the achievement of a high level of productivity.[5] Thus the mills were able to buy labour at the price of the non-stable, but then give it the efficiency of the stable, migrant.

The Japanese case is an instance of the utilisation at low money wage rates of labour which would otherwise have been in a condition of disguised unemployment. It underlines the limitation of the fashionable interpretation of disguised unemployment in relation to economic development purely in terms of the existence of surplus labour, since such an approach leaves out of the analysis the crucial question of the money wage at which labour would be made available to the non-agricultural sector. Economies, even with the same degree of underemployment, differ as regards the money wage at which labour can be put into new avenues of employment, according as the type of labour utilisation varies from one society to another and from one economic activity to another. The nature of labour utilisation can also be influenced by deliberate policy, and with it the money wage rate, and so the rate and level of development.

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workers in the textile industry in Bombay City were 13 as. per day. (*General Wage Census*, 1934, Government of Bombay.)

[1] Plantations in South India, for instance, utilised a type of labour that worked 8–9 months and usually came in the form of two-member families (male and female), both members earning. The wage level in plantations was half that in the textile industry in Madras City, although both were situated equally distant from the main recruiting districts for the plantations. (Cf. Labour Investigation Committee's *Report on Labour in Plantations*, 1944, p. 119.)

[2] Teijiro Uyeda, *The Small Industries of Japan*, 1938, p. 86.

[3] *Ibid.*, p. 89.

[4] Imperial Cabinet Series on Wages, quoted in various issues of the *Oriental Economist*. The average wages for women in the textile industries were less than a quarter of the average wages for men in all industries. (Cf. the difference between the supply price of surplus individual migrants from farm families and family migrants, noted in section I.)

[5] The mills usually entered into contract with the girls' parents (to whom a part of the wages were paid); during the stipulated period of work in the mills, the girls were lodged in hostels within the mill premises. The sanctions of the family system discouraged rapid turnover of labour, for the girls were under obligation to return home with their accumulated savings for their dowry.

Horsley Palmer on Bank Rate[1]

By A B. Cramp

The *modus operandi* of Bank Rate is still one of the unsettled questions of economics. The decisive success of the September measures of 1957, including the increase of Bank Rate from 5 to 7 per cent, in reversing the drain of funds from London, together with the less clear-cut but perceptible contemporaneous check to internal inflation, has tended to revive controversy about the power of Bank Rate itself. The rise to 7 per cent was characterised by some financial journalists as a " nineteenth century " use of Bank Rate; and there have been suggestions in the monetary literature of recent years that the effects of Bank Rate on prices and trade may be considerably weaker in this century than they were in the last.[2] It seems possible that investigations which throw further light on just how Bank Rate did work in its pre-1914 heyday may, apart from their historical interest, assist in the task of deciding between the competing claims of modern champions and critics of this classical weapon of control. The purpose of this article is to make a contribution to this light-shedding process by reviewing the pronouncements made by an acute commentator at that period when it was possible for men to observe the operation of Bank Rate with minds free from predispositions derived from received doctrine: to the earliest years, that is, after the abandonment of the ideal of stability of interest rates.

Before 1822 the rate at which the Bank of England would discount first-class bills of exchange had stood at 5 per cent for half a century. In that year it was reduced to 4, and during the twenties and most of the following decade it fluctuated between the narrow limits of 4 and 5 per cent. The effective beginning of the evolution of Bank Rate as a central banking weapon may perhaps be placed in 1833, in which year the new Bank Charter Act included a clause removing bills with less than three months to run from the restrictions of the usury laws, which since 1713 had imposed a maximum of 5 per cent on the rate of discount. Since bills of longer tenor were not eligible for discount at the Bank, this measure gave the Court greater freedom of manœuvre in its rate policy.

The proximate cause of the new legislation was the evidence given to the Bank Charter Committee of 1832 by John Horsley Palmer, whose term as Governor had been extended to enable him to pilot the Bank through this inquiry and its legislative aftermath. Though little had been heard of Palmer before 1827, his subsequent career was rarely free from that controversy which is invariably aroused by true leaders of thought and action. Known to some of his contemporaries as " the Atlas of the Bank ",[1] his reputation has not shrunk with the passage of time. Sir John Clapham called him " a great Governor "[2]; and, summing up his contribution to the development of central banking, a still more recent writer said that " he lived to see much of what he had fought for accepted and little that he denied achieved ".[3] Here is a man whose views on financial topics command respect. We may be sure that he observed closely the practical operation of the tool which he had helped to forge, and we shall examine his recorded comments on the experience derived from its early use, comments which cover the years down to 1848.

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[1] I should like to thank the Trustees of the Houblon-Norman Fund, who were financing my research during the period when this article was written. I also wish to acknowledge the kindness of Professor R. S. Sayers and Dr. L. S. Pressnell in reading and criticising successive drafts.

[2] Cf. the work of Professor R. S. Sayers, from *Oxford Studies in the Price Mechanism*, 1951, p. 13, to *Journal of the Institute of Bankers*, June 1958, p. 167.

This period coincides roughly with that during which Bank Rate climbed to a position of dominance in the Bank's armoury. This primacy was publicly recognised in 1857, when Deputy Governor Sheffield Neave told a House of Commons committee that " the Court look mainly to the raising the rate of interest as the mode of protecting the bullion ",[4] but was no doubt achieved in practice some years earlier.

In order to show clearly the significance of Palmer's later reactions, it is necessary to sketch the background of expectations which he entertained of a more flexible Bank Rate policy. Although regular open market operations were still a thing of the future, he seems to have been confident of the Bank's power to exert a significant influence on the market rate of discount merely by moving its own rate. This confidence rested less on the Bank's sheer weight in the market (the factor usually emphasised by modern writers) than on its position as " the pivot of the general circulation ".[5] The ability to drive market rate downward lay in the fact that the publication of Bank Rate constituted " an offer by the Bank to create money at that rate ", so that " persons having money already in existence to employ, must necessarily offer to lend it under the Bank's public rate, except in times of actual scarcity or deficiency of money currency ".[6] At these latter periods, market rate could be shunted upward by the withdrawal of loan facilities from the London market, and by creating an interest-differential which would force the country banks into line as a defence against the weight of demand for accommodation.[1]

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[1] Henry Burgess, *The Circular to Bankers*, 21 August, 1840.

[2] *The Bank of England* (1944), Vol. II, p. 62.

[3] J. K. Horsefield, " The Opinions of Horsley Palmer ", *Economica*, Vol. XVI (1949), p. 158.

[4] Commons Committee on Bank Acts of 1844–45, q. 327. B.P.P. 1857, Session 2, X. Subsequent references to contemporary sources may be traced by checking the year quoted against the following table:

1832 Commons Committee on the Bank Charter, B.P.P. 1831–32, VI.
1837 J. H. Palmer, *Causes and Consequences of the Pressure on the Money Market*.
1840 Commons Committee on Bank-note Issues, B.P.P. 1840, IV.
1848 Commons and Lords Committees on Commercial Distress, B.P.P. 1847–48, VIII.

[5] 1832, q. 361.

[6] 1832, qq. 570, 477.

Palmer told the 1832 Committee that experience had shown the connection between short- and long-term interest rates to be tenuous,[2] and it was to the short end of the market that he looked for monetary policy to take effect. He agreed with a questioner who suggested that the substitution of rate increases for credit rationing would enable the Bank to " check the demand for discount gradually ".[3] The wider purpose would be to reverse an external drain on the reserves, and though he was not prepared to commit himself on the length of the time-lag which might be involved,[4] Palmer described the process by which this result would be achieved as follows:

> with the increased value of money there is less facility obtained by the commercial public in the discount of their paper; that naturally tends to limit transactions and to the reduction of prices . . . [therefore] it will be no longer an object to import but the advantage will rather be upon the export, the gold and silver will then come back into the country, and rectify the contraction.[5]

Thus Palmer relied on changes in the trade balance, rather than in capital flows, to restore external equilibrium. Although he did mention the latter factor in 1848, we shall see that his emphasis on the influence of monetary regulation on trade remained.

It must be noticed, however, that Palmer had no notion of using Bank Rate, or any other weapon of monetary policy, to combat cyclical fluctuations in trade. He thought that the power to drive market rates downward by a competitive discounting policy should never be used,[6] while in imposing dearer money with a view to protecting its reserve the Bank should not act " in anticipation of events likely to occur "[7] but, exhibiting that passivity which was an important feature of the Palmer Rule, should delay restrictive measures until the drain had actually commenced.

Under the Palmer Rule the Bank's object was to maintain its security holdings at a relatively constant level so that changes in the bullion reserve would be made at the instance of the public. Although he did not suggest that the usefulness of Bank Rate was limited to any particular system of monetary management, in 1832 Palmer emphasised his hope that a variable rate would enable the Bank to manipulate the volume of demand for discount accommodation in such a way as to ensure the desired stability in the volume of securities. He did not at that time specify whether it was the total volume of demand for discounts in the economy which he thought controllable, or whether he merely expected Bank Rate movements to result in appropriate adjustments in the relative proportions of that demand falling upon the Bank of England and the rest of the money market respectively. That it was the latter alternative which he had in mind is indicated by his statement in 1837 that, in face of the increase in its securities during 1836, " according to the principle laid down in the evidence [of 1832] . . . the rate of interest ought to have been advanced by the Bank in order to throw back that excess upon the market ".[1]

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[1] 1837, p. 37.
[2] 1832, qq. 391 ff.
[3] 1832, q. 160.
[4] 1832, qq. 691–92.
[5] 1832, q. 678.
[6] 1832, q. 563.
[7] 1837, p. 36.

After each of the activity peaks of 1836, 1839 and 1847, Palmer recorded his impression of the impact of Bank Rate on the economy. Before reviewing his verdicts it is desirable to stress the difficulty of isolating the effects of Bank Rate changes in these years. In 1836 Bank Rate rose by two steps from 4 to 5 per cent, and in 1839 it reached 6 per cent, but on each occasion the rate movement was accompanied by other restrictive measures such as the reduction of the tenor of eligible bills. When, in the autumn of 1847, a new record rate of 8 per cent was reached, this " package deal " element was absent but the unusual severity of the crisis introduced complications of its own into the task of tracing the part played by Bank Rate. Unlike some of his contemporaries, however, Palmer regarded the rate as the major causal influence at work.

This is demonstrated by his account of the performance of Bank Rate in 1836. Palmer pointed to a general price-fall on the main commodity markets of the order of 20 to 30 per cent, which had occurred " from the first of July last, when the rate of interest was first advanced ",[2] and also referred to the time " when the pressure was commencing consequent upon the advance in the rate of interest ".[3] We shall advert below to the strong terms which Palmer used to describe the damage to trade caused by the high Bank Rates of 1847. In 1839, by contrast, Bank Rate had been ineffective as a price-regulator. Palmer said that the Bank's object of reducing commodity prices and stimulating exports to enable the exceptional corn imports to be paid for without loss of bullion from the reserve had not been achieved[4]; as a result the Bank had been forced to resort to the expedient of raising a temporary credit in Paris.[5]

In addition to these apparently paradoxical changes in the effectiveness of Bank Rate as a deflationary weapon, Palmer had been disappointed in his hopes of using the rate to administer the Palmer Rule. Though the increase of Bank Rate to 5 per cent in 1836 had successfully " forced additional securities upon many of those country banks that adhered to a lower rate ",[1] he presented statistics showing a progressive rise in the Bank's security holdings throughout the second half of 1836, and confessed that the early returns for 1837 would reveal a continuation of this upward trend.[2] In 1839, similarly, efforts to keep the securities even had failed,[3] though in that case the situation had been aggravated by fears among foreign holders of bills on London that the Bank would fail to maintain the convertibility of its notes.[4] In 1847, the Rule was no longer in operation but it was noteworthy that raising Bank Rate had " tended to an increase of the demand [for discount accommodation] instead of a diminution ".[5] Although Palmer could validly claim that his system " was never intended to apply under any extraordinary events ",[6] it is difficult to avoid the judgment that such events occurred with a frequency which he did not foresee when he propounded the theory to the Committee of 1832.

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[1] 1837, pp. 21–2.
[2] 1837, p. 23.
[3] 1837, p. 16.
[4] 1840, qq. 1390–91.
[5] 1840, q. 1436.

The explanation of this rather low degree of correspondence between Palmer's hopes of Bank Rate and the realised results compels attention to his views on the Bank Rate mechanism, the causal chain which he supposed to link changes in the rate with their observed effects on the Bank's position and on that of the economy. Nowhere did Palmer give a single comprehensive statement of his reasoning, but it is possible to build up a fairly clear picture from the fragments contained in his writings and oral evidence. This has not yet been done satisfactorily. In modern literature Palmer's views on the Bank Rate mechanism have usually been noticed only superficially or ignored. I know of only one exception, and in that case I believe Palmer's position has been misinterpreted.

We have already noticed the rather bald statement of 1832 that commodity prices would be affected because a rise in Bank Rate would cause a reduced " facility " of discounting. What exactly did Palmer mean by this ? It would seem, on the face of it, to be a reference to the influence of the availability of credit rather than its cost, to a change on the supply side of the market. The opposite interpretation has, however, been put forward by Elmer Wood, who wrote that among some observers contemporary with Palmer " there was a growing tendency to emphasize the influence of the ' facility ', or cost, of credit upon the volume of commitments ",[7] as if the " facility " and the " cost " of credit were synonymous terms. Elsewhere, Wood noticed the other possibility: " By facility of credit . . . Tooke means either low rates or the ability to obtain advances on slight security ".[8] Yet in Palmer's case he assumed the former position without argument and, referring to Palmer's statement of 1832, said that he stated " very clearly the view that the market rate affects prices by operating directly upon traders' incentives ".[1]

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[1] 1837, p. 37.
[2] 1837, p. 21.
[3] 1840, q. 1411.
[4] 1848, Lords, q. 838.
[5] 1848, Commons, q. 1945.
[6] 1837, p. 42.
[7] *English Theories of Central Banking Control 1819–1858* (1939), p. 51.
[8] *Ibid.*, p. 57.

The available evidence does not permit of an unequivocal settlement of this point one way or the other. The alternatives, however, are not mutually exclusive, and the substantial question at issue is thus that of the relative weight that Palmer would have accorded to each. A careful reading of his statements suggests strongly that Wood placed the heavier weight on the wrong side of the balance.

In support of Wood's view, it can be said that in 1848 Palmer, defending the repeal of the usury laws against Cayley of the Birmingham School's hostile questions, said that the measure had " given all parties the power of borrowing for their own purposes at any rate of interest they see fit to pay ",[2] and before the Lords Committee he agreed to having heard and believed reports of manufacturers " finding Difficulty in executing Orders . . . in consequence of the high Rate to which they were subject for their Banking Accommodation ".[3] It is permissible to comment on these quotations that the first does not preclude the possibility that credit rationing to less eligible borrowers drove them into a sort of secondary money market at prohibitive rates, and that the second might refer to a phase when prices had already started to fall as a result of the first impact of the rise in Bank Rate. However, taken at their face value, they do broadly support the thesis that any influence of Bank Rate on the state of trade resulted from the importance of the interest element in costs as a determinant of profits. It need only be observed that, as compared with the theory of Sir Ralph Hawtrey, this direct influence was apparently not seen as being confined to mercantile activity with its relatively high proportion of circulating to fixed capital.

Elsewhere in his evidence of 1848 Palmer made statements which, like that of 1832, seem to relegate the profit-squeezing effect of Bank Rate to the background. Although prices fell in 1847, the expected fillip to exports had not materialized; instead, foreign trade on both sides of the account had been brought to a standstill.[4] This was attributed, in a variety of phrases, to the effect of high and rising Bank Rates in " checking credit ", " withdrawing credit " and stopping credit,[5] all of which would be rather odd ways of referring to a diminution of the total *demand* for accommodation. We have, too, already noticed earlier indications that Palmer had not anticipated that the *total* demand would be subject to control, but merely the proportion of that demand which fell on the Bank.

The most decisive evidence against Wood's interpretation of Palmer's position, however, comes from the pamphlet of 1837. Palmer observed that the rise of Bank Rate to 5 per cent had created an effect " far more powerful than the actual advance in the value of money. It was a moral apprehension in all prudent minds that there was mischief abroad ".[1] His description of the repercussions of this " moral apprehension " merits quotation at some length. There was, he said,

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[1] *Ibid.*, p. 53.
[2] 1848, Commons, q. 2199.
[3] 1848, Lords, qq. 768–69.
[4] 1848, Commons, q. 2007.
[5] 1848, Commons, qq. 2043, 2109, 2112.

> a considerable abstraction of bank paper [i.e. Bank of England notes] from London by the issuing banks of the country, which . . . increased the pressure of contraction—and was a prime cause of the pressure upon the money market. . . . In such times it is not only the contraction of money which constitutes the evil, but the consequence which invariably ensues of timidity on the part of bankers generally to grant their customary accommodation, and of those situated in the more distant parts of the country accumulating and locking up reserves three or four times greater than they retain under ordinary circumstances.[2]

Although Palmer described in detail only the reactions of bankers, he spoke of fears engendered in *all* " prudent minds ", and it is plain that such a general weakening of confidence would be equivalent to a revision of expectations among traders leading to an increase in liquidity preference which, supplementing the effects of credit rationing by the banks, would be quite adequate to explain the increased weight of demand for discounts which frustrated the Bank's desire to prevent fluctuations in its security portfolio.

This desire for greater liquidity had repercussions on the balance of external capital movements which Palmer remarked in 1848. The influence of Bank Rate increases on such movements has commonly been analysed as a result either of arbitrage by international bankers and others with funds to distribute between international centres in the most profitable manner, or of the liquidation of short-term credits by foreign borrowers unwilling to pay the new high rates. The source noticed by Palmer fell into neither of these categories. The " pressure " resulting from a rise of Bank Rate, he said, " stops credit, and the British merchant sells his goods for the purpose of carrying on his payments, and brings back his capital at an earlier period than it would come in the ordinary course of trade ".[3] In other words, merchants who found themselves actually or potentially unable to obtain cash for the final liquidation of past engagements hastened to strengthen their liquidity positions by the repatriation of trading capital previously employed abroad. If one may distinguish the two mechanisms which we have been discussing as the " liquidity " and the " cost " effects of Bank Rate changes, this phenomenon represented the extension into the international sphere of the " liquidity effect " which operated so powerfully at home.

Whether Palmer's expectations of 1832 ignored or merely underestimated the strength of the " liquidity effect " is difficult to say.

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[1] 1837, p. 37.
[2] 1837, pp. 39–40.
[3] 1848, Commons, q. 2117.

Certainly he soon came to rely on it to explain the operation of Bank Rate not only during major liquidity crises of the 1847 type but also during those lesser " pressures " which were of much more frequent occurrence. In practice the deflationary effects of Bank Rate increases were far more powerful than he had anticipated, the exceptional stickiness of the price level in 1839 being explicable by reference to the unusually low level of stocks.[1] Experience thus led Palmer more and more to stress the importance, implicit in his evidence of 1832, of confining the use of Bank Rate to periods when, the exchanges being adverse, it was necessary to take measures to protect the reserve. After the 1847 crisis he suggested that even in the case of external drains from " political " rather than " commercial " causes,

> judgment might with propriety be exercised as to the cause and probable duration of the drain, and contraction be delayed until the necessity of resorting to an advance of the rate of interest, or otherwise, be exhibited in order to secure the convertibility of the bank note.[2]

Further, once the adverse exchange had been reversed, the painful operation of the " liquidity effect " could safely be mitigitated by an extension of the Bank's issues[3] at " a moderate rate of interest "[4]: unlike Bagehot, Palmer thought it better to try to remove the cause of " unreasonable timidity "[5] than to impose a fine on it. Thus he supported every measure to protect home trade consistent with the preservation of equilibrium at a rate of exchange fixed within narrow limits; but when the two objectives clashed, the Palmer of 1847 was prepared to give external considerations priority over internal ones, as were the monetary authorities of 110 years later.

In the intervening century, Bank Rate had become a swift and efficient instrument for operating on the balance of international short-term lending. The result of this development, together with the increasing familiarity of the weapon as its use became less of a novelty, was that its operation was no longer subjected to regular, searching inquiry. In particular, the supposed influence on trade, though always an important factor in the Court's deliberations on rate policy, escaped both systematic theoretical analysis and attempts at practical corroboration. When, in the changed world of the inter-war years, efforts were made to reinstate Bank Rate as a regulator of trade, the " cost effect " was brought in; it was amenable to theoretical treatment, and it could easily be built into the existing static framework of classical analysis. Real-world changes had in any case put the " liquidity effect " out of the running: the bill of exchange system under which traders were " terribly dependent on one another's solvency "[6] had almost vanished from internal trade and was of reduced importance in international transactions; the banking system had been strengthened by amalgamations, and improvements in transport and communications had almost removed the possibility of even local panics; the development of deposit banking had long ago rendered of small significance the 1844 Act's restrictions on the Bank's note-issuing powers, and the Bank's monetary management had acquired greater finesse. The conditions were gone in which a rise in Bank Rate could cause a sudden increase in the general desire to obtain and to hold unusually high proportions of what were feared to be diminishing supplies of Bank notes. The " cost effect " reigned almost unchallenged —until empirical research began to cast doubt on its efficacy in affecting the marginal decisions of manufacturers and traders. Yet it dies hard. How far is this due to the persistence in economic folk-lore of tales of pioneer days when Bank Rate *did* affect trade most seriously, but for the quite different reasons described by Horsley Palmer?

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[1] 1840, q. 1473.

[2] Memorandum appended to 1848 Commons Committee evidence.

[3] 1848, Commons, qq. 2028–29.

[4] 1848, Commons, q. 1945.

[5] W. Bagehot *Lombard Street*, 12th edition, p. 199.

[6] R. Hawtrey, *A Century of Bank Rate* (1938), p. 35.

The London School of Economics.

A Theorem on Undercompensated Price Changes[1]

By M. McManus

1. Hotelling [4, pp. 248–52] analyses " undercompensated " price changes in order to show that excise taxes impose an excess burden as compared with direct taxation of equal total yield. Hicks [2, p. 41] and Joseph [5] establish this classical proposition in an equivalent but slightly more direct way for the two-good case. Whereas Joseph and Hicks are only concerned with a single individual, Hotelling tries to relate the argument to the whole economy. Although they write of an " income tax ", Little [6] has shown that only a poll tax would satisfy the required conditions unless the supply of effort happened to be the same with and without the taxes.[2] Rolph and Break [7] prove that the standard result does not necessarily hold good in a general equilibrium situation which contains arbitrary " distortions " other than taxes and subsidies. Moreover, Davidson [1] demonstrates the general invalidity of the usual argument in the case of an individual already subject to excise taxes.

This paper is concerned with a problem intimately connected with Davidson's Case 3. Consider an individual (or a constant cost, no-other-distortions economy) in Situation 0 in which there are no taxes or subsidies, and also in Situations 1 and 2 in which there are two different sets of *ad valorem* and lump-sum taxes and subsidies yielding zero net revenue to the government, given that prices net of taxes and subsidies remain constant. Hotelling's analysis shows that Situations 1 and 2 are each revealed inferior to Situation 0. But no such definite comparison between Situations 1 and 2 is, in general, possible. This must be so, since it is arbitrary which of the two tax-subsidy situations is labelled 1 and which 2. It transpires, however, that Situations 1 and 2 can be ranked in terms of preference if the two sets of changes in the prices are positively proportional to each other, provided certain other conditions are fulfilled. The result in this simple case may be useful when considering more complicated changes which are " almost " proportional. It may also prove possible to derive similar results for other special types of price changes.

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[1] This paper developed out of research undertaken by the author at Professor L. McKenzie's workshop of the Summer Institute on Applications of Mathematics in Social Science Research, organised by the Social Science Research Council and held at Stanford University, California, July–August, 1957. Special thanks are due to Professor McKenzie, Visiting Professor W. M. Gorman, and Mr. P. K. Newman for valuable comments, though the author alone is responsible for the paper's shortcomings.

[2] From the definitions of the p_i, q_i, and m given by Hotelling [4, p. 249], it is clear that his " income tax " is a lump-sum tax and is not levied on earnings from any supplies of labour or other services which may be variable. Thus the only objection can be to his choice of nomenclature.

2. The following situation is examined. It is assumed that the individual is free to and does base his choices on an unchanging complete preference ordering depending only on the amounts of commodities he gives up or receives. Let the variable price vector, $p(t)$, be defined by

$$cp(t) = p_0 + t(p_1 - p_0) = (1 - t)p_0 + tp_1 \tag{1}$$

where c is a constant positive scalar, p_0 and p_1 are arbitrarily chosen and fixed price vectors, and the scalar t takes on all real values.[1] Let all changes in prices from p_0 be *undercompensated*, which means that

$$p_0\, q_0 = p_0\, q_1 = p_0\, q(t) \tag{2}$$

where q_0, q_1 and $q(t)$ are the vectors of the quantities demanded or supplied at the corresponding price vectors, p_0, p_1 and $p(t)$.

Undercompensation of a given price change is not necessarily unique. Suppose it can be and is chosen so that, with (1) and (2) satisfied throughout, total expenditure, $p(t)\ q(t)$, is a continuous function of $p(t)$ for all values of t from 0 to the larger of 1 and the particular value of t under scrutiny. In line with this, suppose that q_1 itself is chosen so that $p_1\, q_1 = p_1\, q(1) = cp(1)\, q(1)$. In this event the situation will be said to satisfy the *Continuity Assumption.*

The *Convexity Property* of the inverse excess-demand functions states that if a set of quantities is optimal at two different sets of prices, it is also optimal at any multiple of any convex combination of these price vectors, provided that the price changes are compensated so as just to keep those quantities budget possibilities. To prove this proposition, let $q_r = q_s$ for two price vectors, p_r and p_s, and consider the budget, $p_h\, q \leqq p_h\, q_r$, where $cp_h = hp_r + (1-h)\, p_s$, $c > 0$. Thus, for any particular such q,

$$0 \leqq cp_h(q_r - q) = hp_r(q_r - q) + (1-h)\, p_s(q_s - q) \tag{3}$$

When $0 \leqq h \leqq 1$, either $p_r(q_r - q) \geqq 0$, in which case q_r is revealed to be at least as good as q, symbolically, $q_r\, W\, q$, or otherwise $p_s(q_s - q) \geqq 0$, in which event again $q_r = q_s\, W\, q$. This proves that $q_r\ (= q_s)$ is also optimal at p_h when $0 < h < 1$.

3. The following theorem is proved in section 5. Suppose that throughout the range under consideration: (*a*) price and quantity changes are chosen so as to satisfy (1), (2), the Continuity Assumption, and the Convexity Property; (*b*) demands and supplies are continuous functions of prices and total expenditure; (*c*) the *Weak Axiom of Revealed Preference* holds, i.e., for any two situations, r and s, either $q_r = q_s$ or else

$$p_r(q_r - q_s) \geqq 0 \Longrightarrow p_s\, (q_r - q_s) > 0 \tag{4}$$

Then $q(t)\, S\, q_1$ (read $q(t)$ is revealed to be strictly preferred to q_1) if $0 \leqq t < 1$, or $q_1\, S\, q(t)$ if $t > 1$, except where $q(t) = q_1$.

What (1) requires is " less or more of the same ". If prices change in a certain way between Situations 0 and 1 then, apart from a scalar factor, some proportion or multiple, t, of the same changes is needed to get from Situation 0 to a different Situation, say No. 2. In two dimensions the theorem is visually apparent. Any three two-dimensional price vectors can be expressed as in (1). In Fig. 1, A, B and C are the chosen points on price lines A′ A″, B′ B″ and C′ C″, and depict Situations 0, 1 and 2 respectively. As t continuously increases from 0 to 1, the absolute slope of C′ C″ continuously increases from that of A′ A″ to that of B′ B″. For $t > 1$, C′ C″ would be steeper than B′ B″ too. All the conditions of the theorem are met if B and C lie on A′ A″ and if, for example, smooth indifference curves exist and are strictly convex to the origin, with the marginal rate of substitution of X for Y continuously increasing along A′ A″ from A to as far past B as is required. The conclusions of the theorem are seen to be satisfied.

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[1] Houthakker [3] uses this price function to trace out " indifference sets ".

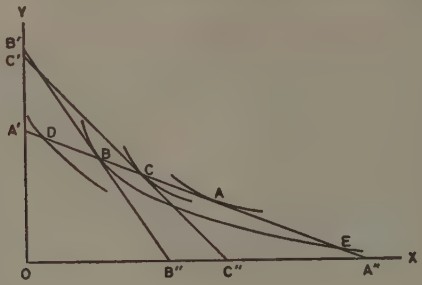

Fig. 1

4. The conclusions of the theorem seem so plausible intuitively that it is tempting to think they might hold true irrespective of the Continuity Assumption and the Convexity Property. Unfortunately, this is not so.

The need for the Continuity Assumption can be seen in Fig. 1, supposing the marginal rate of substitution at D to be the same as it is at C. Therefore the budget line in Situation 2 could pass through D instead of through C if the Continuity Assumption is relaxed. In this event, Situation 1 would be preferred to Situation 2 even though t is less than unity.[1] This case occurs because Y is an inferior good at the relevant prices and low incomes.[2]

On the other hand, Fig. 2 shows that continuity alone is not enough. With the price line getting steeper in absolute terms and moving continuously from A′ A″ through C′C″ to D′ D″, assume that the optimal position can be moved continuously from A through B to D. If the indifference curves between I_1 and I_2 are more and more kinked along DB, further continuously undercompensated increases in the relative price of x can bring the optimum back towards B, B again being optimal for price line B′ B″. Hence higher and higher undercompensated rises in the relative price of X reduce utility up to a point and then increase it again. Instead of this, the result stated in the theorem is obtained by letting B remain the optimal position for all the price lines with absolute slopes between those of C′ C″ and B′ B″, and it is this possibility of a stationary choice for the optimum that is called the Convexity Property.

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[1] By ignoring this possibility, the conclusion stated by Davidson [1, p. 213] for his Case 3 is incorrect. Points A, B and C in Fig. 1 above correspond respectively to points P_3, P_2 and P_4 in his Fig. III. But a point which satisfies the same tax conditions and exhibits the same marginal rate of substitution as P_4 does may also exist between P_2 and L (the point where his dashed line meets the vertical axis), and for this choice the opposite preference ranking holds. Walker [10] wrongly accepts Davidson's result as correct.

[2] Although Davidson [1, p. 209, fn.3] postulates that X is not an inferior good, he places no such restriction on Y. In any case, he seems to imply that the results are unaffected by the presence of inferiority.

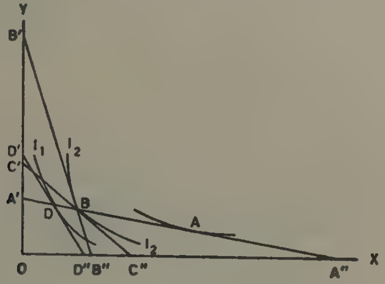

Fig. 2

It is not always possible to undercompensate an arbitrary set of price changes. In Fig. 1 for example, very large increases in the relative price of X could not be undercompensated if the marginal rate of substitution along A′ A″ nowhere exceeds that at B. More importantly, in quite ordinary cases the Continuity Assumption cannot be satisfied no matter how the compensatory changes in income are chosen. In Fig. 1, for instance, there would be a discontinuity in undercompensatory movements in total expenditure at the price ratio OC′/OC″ if the marginal rate of substitution remained constant (or decreased) for some distance along CB before increasing again near B.

5. To prove the theorem, some simple properties required later on are first obtained. From (1) and (2), $cp(0)\,q(0) = p_0\,q(0) = p_0\,q_0 = cp\,(0)\,q_0$. But $p(0)\,q(0) = p(0)q_0$ and $p_0\,q_0 = p_0\,q(0)$ together contradict (4) if $q(0) \neq q_0$. Hence $q(0) = q_0$. Similarly $q\,(1) = q_1$, since part of the Continuity Assumption demands that $cp(1)\;q(1) = p_1\;q(1) = p_1\;q_1 = cp(1)\;q_1$. Essentially, this is the proof given by Samuelson [8], [9, pp. 111–12) that his Weak Axiom implies that the excess-demand functions are single-valued and homogeneous of degree zero in prices and total expenditure. Next, (1) shows that $p(t)$ is a continuous function of t. By the Continuity Assumption, therefore, $p(t)\;q(t)$ is

continuous in t. Hence $V_2(t) \equiv p(t)\,(q(t) - q_1)$ is a continuous function of t. Finally, since both prices and total expenditure vary continuously with t, so does $q(t)$, because the excess-demand functions are continuous and single-valued. Therefore, $V_1(t) \equiv p_1\,(q(t) - q_1)$ is continuous.

Now consider values of t in the unit interval, $0 \leqq t \leqq 1$. Let t_m be the greatest lower bound of the values of t for which $q(t) = q_1$. By the Convexity Property, $q(t) = q_1$ for all t satisfying $t_m < t \leqq 1$, so such values require no further analysis. If $q(t) \neq q_1$ for all $cp(t) \neq p_1$, then $t_m = 1$ since $q(1) = q_1$. If $q_1 = q_0$ then $t_m = 0$ and $q(t)$ does not vary at all for t in the unit interval. Henceforth, therefore, assume that $t_m > 0$, so that $q_1 \neq q_0$.

It follows from (1) and (2) that

$$cV_2 = cp(t)\,(q(t) - q_1) = (1 - t)p_0\,(q(t) - q_1) + tp_1\,(q(t) - q_1) = tV_1 \quad \ldots\ldots (5)$$

for all t. If $0 < t < t_m$ then $V_2 \neq 0 \neq V_1$, because otherwise both V_1 and V_2 would have to be zero to satisfy (5), and this would contradict (4), since $q(t) \neq q_1$ in this range. Together with (5) and the continuity of V_1 and V_2, this shows that if $0 < t < t_m \leqq 1$ then either V_1 and V_2 are both positive or else both are negative. But, applying (4) to (2),

$$V_1(0) = p_1\,(q(0) - q_1) = p_1\,(q_0 - q_1) > 0 \ldots\ldots (6)$$

By the continuity of V_1 at $t = 0$, therefore, $V_1 > 0$ for $0 \leqq t < t_m$. Hence

$$V_2 = p(t)\,(q(t) - q_1) > 0, \qquad 0 < t < t_m \leqq 1 \ldots\ldots (7)$$

which implies that $q(t)$ is preferred to q_1, as was to be proved. Similarly, either $q(t_m) = q_1$ or else $V_2(t_m) > 0$ and $q(t_m)\,S\,q_1$.[1]

Perhaps the simplest way to prove the theorem for a value $t = b > 1$ is as follows. From (1), $bp_1 = (b - 1)\,p_0 + cp(b)$. Thus (1) can be transformed to

$$bcp(t) = b(1 - t)p_0 + t(b - 1)p_0 + tcp(b) = (b - t)p_0 + tcp\,(b) \ldots (8)$$

Hence, in view of (8) and (2),

$$bW_1(t) \equiv bcp(t)\,(q(t) - q(b)) = (b - t)p_0\,(q(t) - q(b)) + tcp(b)\,(q(t) - q(b)) = tcp(b)\,(q\,(t) - q(b)) \equiv tcW_2(t) \ldots (9)$$

The procedure is now similar to that used before. Either $q(b) = q_1$ or, since $cp(1) = p_1$, $q(t) \neq q(b)$ for all non-negative t no greater than some value $t_m \geqq 1$. By (2), $W_1(0) = p_0(q_0 - q(b)) = 0$, which implies by (4) that $W_2(0) = p(b)\,(q_0 - q(b)) > 0$. Moreover, when $0 < t \leqq t_m$, $W_1 \neq 0 \neq W_2$ to avoid contradicting (4). Hence, since both W_1 and W_2 are continuous, they are both positive for $0 < t \leqq t_m$. In particular, $0 < W_1\,(1) = p_1\,(q_1 - q(b))$, and so $q_1\,S\,q(b)$. Q.E.D.

6. The case where all the conditions of the theorem hold but where $t < 0$ is now briefly examined. If $q_1 = q_0$, then, in view of (2), either $q(t) = q_1$ or else $q_1\;W\;q(t)$. Now suppose $q_1 \neq q_0$. Then $q(t) \neq q_1$, since $q(t) = q_1$ when $t < 0$ implies by the Convexity Property that $q_0 = q_1$. It follows that $V_1 \neq 0 \neq V_2$ because of (5) and (4).

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[1] It may be that, under the conditions stipulated in the theorem, the set of t for which $q\,(t) = q_1$ is closed, so that t_m is a minimum value and $q(t_m) = q_1$.

Hence $V_1 > 0$ because of (6) and the continuity of V_1 at $t = 0$. From (5), therefore, $V_2 < 0$. But $p_1\ (q(t) - q_1) > 0$ and $p(t)\ (q(t) - q_1) < 0$ reveal nothing about the preference ranking of q_1 and $q(t)$. In the two-dimensional example, the C′ C″ of Fig. 1 would be flatter than A′ A″ for negative t. C would be between A and A″, and so could have any of the three possible preference relationships to B depending on its geometric relationship to the point E where the indifference curve through B cuts A′ A″ again.

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Book Reviews

The Growth of Industrial Economies. By W. G. Hoffmann. Translated by W. O. Henderson and W. H. Chaloner. Manchester University Press. 1958. xiii + 183 pp. 25s.

Possibilities of Economic Progress. By A. J. Youngson. Cambridge University Press. 1959. x + 325 pp. 32s. 6d.

It is natural that current concern with the theory of economic development and with the prescription of policies for underdeveloped areas should encourage a reconsideration of the record of the past. Since, by now, an appreciable number and variety of countries have experienced the achievement that is commonly accepted as rapid economic progress, it may be worth while to enquire whether their experience has any uniformity, whether it offers any clear indication of how the trick has usually been worked and how it may most effectively be worked in new places. Both the books under review are concerned with questions of this kind, though they differ greatly from each other in their approach to the subject, in their conclusions and their method of formulating them, in content, and in quality.

The word " possibilities " in Professor Youngson's title is indicative of his more cautious and conditional treatment of his theme, whereas Dr. Hoffmann's title suggests something much vaster. A comparison of the two books, however, shows that, though Dr. Hoffmann's conclusions are more confident and more universal, they are based on a much narrower consideration of economic change, and one which brushes aside many fundamental difficulties without doing anything to resolve them. It would have been less misleading if the title of his original, rather shorter, German version (published in 1931) had been literally translated as *Stages and Types of Industrialization*, for Dr. Hoffmann never considers as a whole the economy of any industrial country. He is concerned only with changes within the industrial sector of the economy. Consequently he makes it impossible for himself to explain why industrial economies grow, and he can do no more than describe some characteristics of their industrial structure. Nevertheless he goes so far as to present a universal law of growth. He divides the years since 1770 into four periods and compares the experience of countries beginning their industrial revolutions in each of these periods, in order to discover the changes in the ratio of net output of consumer-goods to that of capital-goods. He concludes that, whatever the date at which their industrial revolutions began, all industrializing countries have followed a common pattern of growth through three stages. In the first stage of industrialization consumer-goods industries have predominated, the ratio of the net outputs of consumer-goods and capital-goods industries being 5 (±1):1. In the second stage capital-goods industries were growing the faster of the two and the ratio was 2·5 (±1):1. In the third stage the ratio was 1 (± 0·5):1, and some countries have reached a fourth stage with a still lower ratio.

Is this true and is it significant? The numerical ratios presented for different countries at different times are unlikely to be at all closely accurate. To calculate values of net output from the scanty data available in many instances, Dr. Hoffmann makes assumptions on which the most favourable possible comment is a raised eyebrow; and, though he admits there are pitfalls in international comparisons based on figures differently compiled and differently classified, in practice he ignores them. Nevertheless, the general trend of change to which he draws attention is correct, on his definition of terms, even if his precise figures are suspect. It does not, however, throw much light on the nature of economic growth. In the first place, it does not indicate how the first stage of industrial growth differs from pre-industrial conditions. It is not clear how Dr. Hoffmann decides on the date at which the industrial revolution began in different countries. Since his scheme has it beginning in Switzerland and the U.S.A. at an earlier period than in Belgium and France, and has Greece completing the first stage between 1861 and 1890, his judgment looks odd. In the second place, his definition of terms is peculiar. Throughout the book, consumer-goods are things sold direct to the public, capital-goods are things sold to other industries. Thus, explicitly, railway freight wagons are capital-goods but passenger-coaches are consumer-goods; and, implicitly, the same locomotive is a capital-good when it is hauling a freight train but becomes a consumer-good when pulling a passenger train. Given these definitions one can vary the consumer-goods/capital-goods ratio quite arbitrarily by classifying commodities either as semi-products in course of further manufacture or as finished goods to be sold to another manufacturing industry. All that Dr. Hoffmann's universally declining ratio tells us is that a steadily increasing proportion of manufactured output consists of fairly complex goods incorporating many separately manufactured parts and created with the aid of many types of machine. This is an elementary matter of common observation, affecting both early and late industrialized countries and causing some of the latter to gallop through Dr. Hoffmann's first two stages. But it does not show us how and why countries become industrialized, nor what are the most auspicious conditions for further industrial growth.

To turn to Professor Youngson's work is to be carried to an obviously higher level of intellectual distinction. Far from ignoring the complexity of the factors involved in economic change, and the difficulties (both logical and practical) of identifying, defining, and measuring them, he devotes the first third of his book to making them as explicit as possible. From this discussion he is led to select certain related influences that appear specially likely to affect economic progress and he uses his ideas about them, very flexibly, in considering four cases of accelerating economic progress: Britain, 1750–1800; Sweden, 1850–80; Denmark, 1865–1900; and the Southern United States, 1929–54. Finally, he tries to sort out the common and the individual elements in these four examples of progress, and to decide what guidance, if any, they can offer to those responsible for policies of economic development in backward areas. In the four historical cases he concludes that the chief similarities were the advantage of international contacts, the importance of agricultural reform, and the complexity and significance of inter-trade and inter-sectoral relationships. He also adds that adequate capital was forthcoming with perhaps surprising ease through the re-investment of profits, extra work, and the ability of primary producers to earn foreign exchange; that major acts of investment in anticipation of and mainly for the furtherance of long-period growth were unexpectedly inconspicuous in comparison with the multitudinous smaller investments of a more humdrum kind; and that the influence of different rates of population growth has varied greatly with the setting in which they occurred. For present policy in underdeveloped areas he makes four tentative suggestions. The first is that the extension of international contacts is likely to be more beneficial than economic nationalism. The second is that governments might do better to concentrate on improving the general equipment of economic life, e.g., by improving fiscal administration, banking and education, rather than to put most of their effort into specific productive schemes. Thirdly, he insists with unquestionable truth, that economic progress is not just industrialization but that agricultural improvement is also of the first importance. His last point is that governments should pay more attention to making the social system more tolerant, flexible and sympathetic to economic progress.

Any of his main arguments could usefully be discussed at length, but in most cases it is clear from the author's own comments that he is aware of and anxious to admit whatever uncertainty there is about their validity and general applicability. A question that may be asked, however, is whether historical enquiry at the level attempted here is a safe basis for the discussion of economic progress. The case studies are not detailed pieces of original research, though the Danish material will be unfamiliar to English readers. Each of them has the length and content of a couple of lectures of the kind contributed to a general course of economic history for economics students—lectures which summarize, re-arrange and comment upon the detailed work of others. They are excellent examples of their kind, and it is reassuring to those who often have to do the same sort of thing that their kind can be as informative, illuminating and stimulating as it is here. This is not to say that it is adequate in all respects, only that it is clearly useful. Brevity is no doubt the main reason why, when the complexity of possible demographic influences has been displayed, no attempt is made to unravel them very far, though this is necessary for the understanding of some of the most important questions about the possibility of economic progress today. A less accountable feature of Professor Youngson's book is that capital accumulation and investment plays a much larger part in his preliminary theoretical and general discussion than it does in the subsequent case studies and conclusions. This may be a sign of a misplaced focus of attention. Capital requirements and capital ratios are tricky things to play with in the abstract, trickier still to measure reliably in specific cases, trickiest of all to account for historically. But, tantalisingly unpredictable and only imperfectly knowable as they may be, they do illuminate something fundamental to economic change. It is worth looking in specific cases to see whether changes in the volume of investment and its apparent productivity show any possible association with changes in the rate of population growth and the age structure of the population, in the main types of products and the techniques of making them, in the location of population and resources, in the opportunities to buy or borrow the output from the capital equipment of other countries. To some extent, the small attention to such questions in the present book may reflect the limitations of the cases chosen for study. England before 1800 and Sweden before 1880 had not proceeded far along the road of economic progress, and their early progress would not attract much attention if it had not continued to be greatly extended. In their subsequent achievement the role of increasing investment, especially long-term growth investment, became for a time much more evident, and this appears to be relevant to *sustained* economic progress in most places.

Thus, in a way, we are brought back nearer to the theme which Dr. Hoffmann tried to pursue, but not at all in his narrow sense. He had a sound instinct in seeking to demonstrate the significance of capital-goods in industrial growth and he earns the credit of a pioneer at a time when his line of study had not become fashionable. But he failed to define his concepts so that they are significant for the real economic world, he glossed over the problems of measurement involved, and he neglected to put the capital-goods industries in that context of change in the whole economy which alone can make sense of their activity and growth. Consequently, he took the study of industrial growth up a blind alley.

Professor Youngson has much more to offer, though he would not claim to have given the study of economic progress a thorough historical underpinning. Slight changes in the focus of attention and a different choice of historical examples (including cases of lack of progress) might well alter the recommendations for future policy. Historical cases could be found that are more nearly parallel to contemporary conditions in many areas newly seeking economic progress: contrasted studies of advance and its absence in northern and southern Italy in the late nineteenth century might be more immediately relevant than any cases used here. But what Professor Youngson has written has the merit of forcing one to ask basic questions: is any general study of economic progress possible and worth while ? Can economic history contribute to it and be clarified by it ? He has done enough to suggest that these questions can be answered affirmatively, even though the existence of economic progress can never be proved absolutely, even though (whatever definition of it may be adopted for practical purposes) its measurement can never be exact, and even though historical illustrations of it must be still less precise and sometimes conflicting. His book is an unusual one and deserves to promote emulation.

W. Ashworth.

Economic Forecasts and Policy. By H. Theil (assisted by J. S. Cramer, H. Moerman and A. Russchen). North-Holland Publishing Company, Amsterdam. 1958. xxxi + 562 pp. £5.

For the last decade econometric models have been employed by the Dutch Central Planning Bureau. Professor Theil's book provides a retrospective examination of the predictive quality of such models (and of others), and an analytical discussion of their possible use in resolving the more fundamental problem of decision-making. Apart from these two aspects the book contains much statistical methodology aimed, in particular, at the estimation of systems of simultaneous equations. It is in fact a report of the research carried out since 1953 when the decision to investigate the possibilities of a new Dutch econometric model was taken.

In the first half of this book annual forecasts derived from Dutch models, for the years 1949–51, 1953, 1954 and 1955, and from Danish, Norwegian and Swedish models, for the years 1949–52, are meticulously compared with actual outcomes. Passing from the macro-economic to the micro-economic Professor Theil next considers the forecasting powers of entrepreneurs. Monthly predictions relating to the German leather and shoe industry, collected as part of the Munich Business Test during the years 1951–53, are also confronted with actual results. These two intensive analyses reveal that the forecasts of both central economic planners and entrepreneurs display a marked characteristic: the underestimation of changes. Indeed, for the macro-economic annual forecasts of the many variables described by the model, actual changes were, on the average, underestimated by as much as 30 per cent. The results obtained from the accuracy analysis of the monthly entrepreneurial predictions are more diffuse, but the tendency to underestimate changes emerges, so much so that this particular phenomenon is put back under the microscope of the econometrician in a subsequent chapter. Additional forecasts (the American Dun and Bradstreet surveys and the O.B.E.-S.E.C. surveys, and the M.B.T. results for the German textile industry) are also shown to display this same tendency, which by now seems to have become inexorably inevitable.

The latter half of this book is, I believe, the first published attempt to apply the theory of decision-making, in the face of uncertainty, to the problems of central economic planning. The technique of maximising a social preference function, given linear constraints describing the economic system, has already been explored for example by Frisch and Tinbergen, but account was not explicitly taken of the uncertainty of the forecasts which need to be made. It required the proof of the theorem of " certainty-equivalents ", set out in this book, before the additional problem of uncertainty could be handled. This theorem states that if a set of forecasts, of a given variable, can be attributed a given probability distribution, then it is only the expected or mean value of the forecast which need be considered to obtain the optimal decision. This is a happily convenient result, for the *formal* application of decision theory may then proceed as though the forecast(s) were exact; the symmetry of a quadratic preference function is, however, necessary for this to hold—not that this seems to constitute, in fact, a severe limitation. The static case, where the decision and its implications are considered for a single period, is discussed fully, but it seems to the reviewer that the more interesting dynamic situation might have been explored a little more.

It is hardly necessary to comment upon the statistical methodology. Anyone who has read Professor Theil's works in recent years will recognise much of his ingenuity in this book, which provides a convenient collection of many of his articles.

In conclusion it should be remarked that this book is not easily read. The author himself suggests that those mainly interested in results should read the concluding chapter at the outset. But even when reading the empirical analyses of the first part it is advisable to refer, from time to time, to the conclusions, if only to light the way through a superabundance of facts and figures. The book might have been better presented as two: one dealing with the accuracy analysis of forecasts, the other containing the formal discourse on decision theory for economic planning. But this is a technical report and will be welcomed both by advocates and sceptics of the use of econometric models.

L. A. DICKS-MIREAUX.

International Economic Papers, No. 8. THE INTERNATIONAL ECONOMIC ASSOCIATION. Macmillan & Company Ltd. 1958. 240 pp. 21s.

This eighth volume of the International Economic Papers contains one outstanding contribution. It is entitled " The Role of the Banking System in a Macro-Economic Model " and is by a young Norwegian economist, Leif Johansen. In this paper, for the first time since Kragh's well-known article in the *Review of Economic Studies* (1949–50), an attempt has been made to examine the workings of a static Keynesian model in which banks have not been assumed away. The results are illuminating and seem to me a first step towards a really literate analysis of a monetary policy. The model has the virtue that it can be " dynamised " very easily and that the special features of British (as opposed to the Scandinavian) monetary institutions can be readily incorporated in it. The author's simplicity of style and coolness in the handling of the various parameters is impressive. The article should be widely read.

The rest of the papers are not quite in this class, although most, as we would expect, are highly competent. One does, however, gain the impression that the editors are overly concerned with works of " piety ". Thus reprinting Stackelberg's paper on discriminating monoply is not very useful—most students in an elementary course of mathematics for economists might nowadays be expected to know his result. Ferrara on the Physiocrats must, I feel, also leave many readers cold. Much of what he had to say, flowery and in many ways pleasing as his style (in translation), may be, is only moderately right. Occasionally it looks as if at any moment he will anticipate Wicksteed's *Co-ordination*, but he does not. On the other hand Alfred Weber's " Location Theory and Trade Policy " (1911) was worth reprinting. It is interesting and, since location theory is relatively neglected in the Anglo-Saxon literature, it will almost certainly teach the reader something new. In particular Weber's discussion of the view that " the normal and fundamental situation . . . is an even distribution of production over the whole area and international division of labour is a deviation therefrom " (p. 138) is both valuable and stimulating.

If there is too much "piety", the reprinting of papers by East European economists is entirely to be welcomed. Perhaps there should be even more of this. Professor Stefan Varga's paper, "Money in Socialism", is fascinating. He writes with great freedom and is highly critical of contemporary Marxist theory on this topic, as well he might be. For it seems that orthodox Russian economists believe that the exchange value of money is determined by and entirely depends on its gold backing, and that crises in capitalist countries are caused by the hoarding of gold on the part of capitalists. Varga has little difficulty in disposing of this nonsense. What emerges is that money in socialism is much like money in capitalism, except that governments may fix and control money prices. Western work on suppressed inflation seems to have had considerable relevance to the post-war Russian situation. Rumen Janakieff's (Sofia) "Critical Comments on the Use of the Gross Production Index" is also of exceptional interest. In economies where targets are set to individual enterprises and their efficiency is judged on the basis of certain indices, the construction of these becomes of peculiar importance. Janakieff shows that the gross production index, which simply measures the total output of a plant without deducting "resources used up", widely employed in Eastern Europe, tends to a mis-allocation of resources and to great difficulties in control. (For example, products with low labour content are favoured by managers since they yield a high "gross product per worker".) After a most competent and illuminating discussion, Janakieff advocates the use of what is essentially a "value-added" index. It is interesting to note how the Marxian theory of value seems to have bedevilled Soviet planning.

Amongst the other papers is one by Bentzel "On the Aggregation of Production Functions", which summarises the literature and should prove useful. Erich Schneider contributes "Income and Income Distribution in Macro-economic Theory" which is Kaldorian and somewhat unconvincing. Jürg Niehans' "An Index of the Size of Industrial Establishments" is most illuminating. He proposes an index which in crude form reads $\frac{\Sigma a_i^2}{A}$ where a_i is the number of workers in the i^{th} establishment and A is the total number of workers in the "industry". It is obviously not "perfect" but Niehans' reasons for preferring it to other possible ones are convincing to a non-specialist. Mr. Bye's "Self-financed Multiterritorial Units and their Time Horizon" deals with the economics of international oil companies and other similar concerns. The subject is important but the paper rather inadequate. It is classificatory and it leaves the crucial "time-horizon" concept distinctly under-analysed.

There remains one more paper. It is by Professor Lundberg and is entitled, "The Stability of Economic Growth". It was written in 1950 and, while Lundberg has rendered signal services to economics, this seems inadequate reason for reprinting this particular piece. From Colin Clark's "statistical gangster methods" and Mr. Harrod's "typical tendency of British economists to be, as Myrdal says, 'unnecessarily original', to *Value and Capital* whose 'sterile problems and . . . dead logic have already bored to tears ten generations of students and a generation of teachers' ", it is unrelieved tedium. For all Mr. Lundberg has to say in between is that the models are imperfect, that the evidence is ambiguous and difficult to find and that for these reasons we should take neither models nor statistical exercises *à la* Kuznets or Clark too seriously. In particular, attention should be paid to such qualitative matters as the inducements to innovate. Who has ever doubted this? Authors do sometives write as if their models were final and of immediate applicability; this is irritating. It is however not an adequate excuse for long and boring disquisitions.

Lastly one would like to renew the plea to the editors to get someone who can translate from the Japanese, as soon as possible. There are many important papers to be had from that source, judging by the few one has seen in English.

F. H. HAHN

An Approach to the Theory of Income Distribution. By SIDNEY WEINTRAUB. Chilton Company, Philadelphia. Bailey Bros. & Swinfen Ltd. 1958. x + 214 pp. 52s.

One of the limitations of the marginal productivity theory of distribution is that it does not admit of any underemployment equilibria in the Keynesian sense. The productivity schedules, even if in aggregative form, correspond to the position of general equilibrium and there is no interaction between the factor prices which those schedules determine and the aggregate demand which determines the schedules. There is, in other words, no connection between the theory of the distribution of income and the theory of its determination.

To establish such a connection is the main purpose of Professor Weintraub's book. The general line of the argument is as follows. The economy consists of a number of firms which are supposed to be fully integrated.

With a given wage rate the cost curves of the firms are thus also given, and there is a certain relationship between prices which the firms expect to receive for their products and the firms' outputs. This, on certain assumptions, can be translated into a relationship between expected aggregate proceeds and aggregate employment (the aggregate supply schedule). To each combination of proceeds and employment corresponds then a certain aggregated demand, determined by various institutional factors, such as the level of fixed incomes of rentiers, taxes, etc. The economy is in equilibrium when the aggregate demand is equal to the aggregate supply. Employment and national income are thus determined by the equilibrium conditions of individual firms, and the factors which govern the behaviour of firms are in turn determined by employment and the level of national income.

Having established these general relationships the author proceeds to investigate such problems as the effect on relative shares of changes in productivity, in capital accumulation, monopoly power and the general level of employment. The analysis is a mixture of elementary calculus and far-fetched " common sense ". The typical procedure is to differentiate the formula determining the quantity in which the author is interested, and to find out what might be the sign of the differential coefficient. And as this usually depends on the relative magnitudes of other coefficients, as to which numerical values we have no information at all, the result depends ultimately on intuitive judgment and speculation.

The book contains also separate chapters on wages, rents and profits. With the exception of the theory of wages the points discussed there are only very loosely connected with the main theme of the book. The theory of wages is more or less as follows. To each level of money wages corresponds a certain aggregate supply schedule and a certain aggregate demand schedule. They determine the level of employment corresponding to that wage. The same applies to other levels of wages. The demand for labour is thus a function of wages. The supply function is given by workers' reactions to changes in money and real wages, implied in the supply function. Both of them together determine the equilibrium level of wages (in money).

The above theory of wages and the general theory of distribution from which it derives are equilibrium theories in which there is no place for any dynamic considerations. Nothing is said about how equilibrium is reached, about time lags or differences in speed with which the adjustments are made. This is no doubt a very serious weakness of a theory which refers to short-run phenomena. In the short run complete equilibrium of the whole economy may never be reached, and what can be observed may be only successive stages in a continuous process of change.

There are also points on the margin of the main theme of the book, to which many readers may take exception. It is, for instance, not very easy to agree to the dismissal of the loanable funds theory on the ground that what it amounts to is " a tortuous study of income flows and the splintering off of income streams of saving and investment which, in the final telling, may have little to do with the interest level ". Nor is the treatment of the liquidity-preference theory, in which demand for money depends on its supply, likely to be accepted without a protest.

Taken as a whole Professor Weintraub's book represents a serious attempt to answer important questions, and although we may disagree with some of the procedures it deserves proper attention.

S. A. OZGA.

Dynamic Programming. By RICHARD BELLMAN. Princeton University Press. Oxford University Press. 1957. xxv + 342 pp. 42s.

This book treats multi-stage decision processes. These occur in a variety of applications: in inventory and production control, input-output analysis, investment planning, control system engineering, sequential testing and design of experiments in statistics. They also occur in card games, such as in the bidding system in contract bridge and in poker. In each case we envisage a system of some sort, characterised by being in one of a number of states, which changes as the system evolves through a succession of stages. At each stage there is a choice of a number of decisions which will affect the next state assumed by the system. The object is to maximize (or minimize) some function (the criterion function) of the succession of states, i.e., to find an optimal policy.

The unity of the book resides in the approach to these problems. The author propounds a single approach, the theory of which he originated and for which he has coined the term " dynamic programming ": it indicates a mathematical method rather than an area of application. It is based on his " principle of optimality ": an optimal policy has the property that whatever the initial state and initial decision are, the remaining decisions must constitute an optimal policy with regard to the state resulting from the first decision. Since the invariant property required of processes in order that this principle may apply is not clearly stated, the principle itself could perhaps be taken as defining the class of processes under consideration. It would, however, be interesting (by way of exceptions to prove the rule) to know of some processes to which it does not apply. No such examples appear to be given.

Application of this principle yields a functional equation, the solution to which determines also an optimal policy. The book treats techniques for the solution of such functional equations occurring in the applications. The problem tackled by dynamic programming can be thought of as one of maximizing a function subject to constraints, and Bellman's method (like linear programming) provides an alternative to that of Lagrange multipliers, applicable in circumstances where the latter is not feasible. It has the advantages of revealing the structure of the optimal policy, and also of providing a computational method for determining a solution numerically by successive approximations.

Chapters 1 and 2 treat two simple examples, and in Chapter 3 the approach to these is generalized. In Chapter 4 a number of general existence and uniqueness theorems for solutions of the functional equations are established. In succeeding chapters dynamic programming is applied in turn to the optimal inventory problem, " bottleneck " problems involving interdependent activities, problems in the classical calculus of variations, multi-stage games and, finally, Markov decision processes. An important feature is the large number of unworked exercises and research problems, many of which are of great interest.

The book is directed towards a wide audience: mathematicians, economists, statisticians, systems engineers and operational researchers. It is written at the advanced calculus level. The author has a lively style, and the mathematics is nowhere very deep. However, the reader will often require Bellman's special type of pioneering zeal to be able to persevere to the end of an argument—otherwise he may find the going difficult in places and (dare one admit it ?) tedious. The trouble is that the type of mathematical problem posed in this book is such that it is only under excessively simplified conditions that one can ever hope for pleasing analytic solutions.

F. G. FOSTER.

Microeconomic Theory: A Mathematical Approach. By JAMES M. HENDERSON and RICHARD E. QUANDT. McGraw-Hill Publishing Company Ltd. 1958. xii + 291 pp. 58s.

Although the place of mathematics in the study of economics has been under dispute for at least a century, only in the last two decades or so has the issue excited continual controversy. While no broad agreement has yet been reached, it is surely true that graduate students and academic staff, if only in an endeavour to keep abreast of the journal literature, are increasing their efforts to become more intimate with the popular mathematical techniques applied to their subject.

There are, at present, several well known introductory textbooks which aim to provide the non-mathematical economist with some necessary groundwork. But the continued advance in the application of mathematical methods to economics, and in fact to the social sciences generally, has created a demand for introductory texts to particular branches of mathematics, such as topology, matrix algebra, symbolic logic, differential and difference equations, the employment of which in the service of the social sciences is likely to grow with the passage of time. There are, of course, numerous treatises by competent mathematicians in such fields, and economists should, and no doubt do, find occasion to refer to them. But there is an art in setting up economic problems in a suitable mathematical form, an art to which the manipulation of mathematical expressions is of secondary importance. Any book which by virtue of lucid exposition and patient illustration succeeds in imparting a feel for the application of the particular mathematical techniques it contains is assured of a good market.

A mathematical treatment of some branch of economics, however, may serve a similar purpose, especially if the treatment is illustrative of one or two particular mathematical tools. In the book under review little beyond first-year calculus is required, but it is used with untiring skill to cover a host of related subjects in the theory of consumer's behaviour, of the firm, of market equilibrium, of monopolistic competition, and of welfare economics. A brief mathematical appendix serves to refresh the reader's memory though, as the authors point out, it is inadequate for readers unacquainted with the calculus.

The mathematical treatment of economic theorems moves cautiously, a step at a time, and, more noticeably in the first half of the book, two variables doing frequent duty for n. To ensure familiarity with the propositions under discussion, standard diagrams are made use of and the general conclusions often illustrated with specific functions—a bit wearisome at times, but for those in earnest not, perhaps, more than is necessary.

It is not surprising in view of the number of topics covered that there is plenty to disagree with in points of economic interpretation. But there is also occasional evidence of careless thinking; for instance, in the footnote on page 22, which tells us that consumer's surplus has failed to result in notable advances since " it depends upon the assumption of cardinality ", in the proposition on page 97, that the long-run supply curve of the industry in perfect competition (like the short-run supply curve) is the sum of the long-run supply curves of the individual firms, or in the statement on page 211, that it is a " necessary and sufficient " condition for an efficient allocation of resources that all markets be perfectly competitive. Furthermore, despite the editor's claim that " many clarifications and advances " are suggested by this volume, the economics graduate will look in vain for anything new. Indeed, conceived as a treatise in economics proper, it is a decidedly sedentary effort: no methodological issues are raised; no points of controversy discussed—a rather uninspired exposition of a patchwork of received dogmas.

And yet this is not at all important if the chief purpose of the book is borne in mind: to enable the economist with a smattering of calculus to acquire facility in its application to a range of related economic problems. It can therefore be recommended wholeheartedly to third-year specialists and graduate students anxious to come to grips with the standard mathematical treatment of this crucial branch of the subject.

E. J. MISHAN.

Airline Price Policy: *A Study of Domestic Airline Passenger Fares.* By PAUL W. CHERINGTON. Harvard University Press, Boston. Bailey Bros. & Swinfen Ltd. 1958. xx + 471 pp. 60s.

Professor Cherington caused quite a stir in the international aviation world last year with a report to President Eisenhower on the economics of the \$2·8 billion re-equipment programme being undertaken by the U.S. airlines. In this book he has set out to answer some important questions about a closely related subject, the fares policies of the U.S. domestic airlines. His four main questions are: have these fares policies been aimed at the right objectives, have they been adequately studied by managements, has their regulation by the Civil Aeronautics Board been beneficial, and what changes are required ?

To answer these questions Professor Cherington made a large number of " case " studies which analysed the action taken by airlines faced with specific pricing problems. These individual studies show that he was able to get remarkably close to the thinking of the managements concerned. Detailed financial and statistical data were made available to him, and these case studies are fascinating reading to anyone deeply interested in the peculiar problems of airline price-making. But the author has wisely consigned them to chapter appendices where they need not interrupt the main flow of the narrative.

For U.S. readers the appearance of this book was particularly timely because the Civil Aeronautics Board was, and still is, engaged in a comprehensive public inquiry into the same subject. This " General Passenger Fare Investigation ", which the C.A.B. started in May, 1957, was originally concerned with alleged excessive earnings of the airlines. Adverse traffic results in the winter of 1957/58 changed the picture, however, and the Board was obliged to allow interim fare increases early in 1958. Most of the airlines thought that these increases were inadequate, and higher fares are still under consideration. Whether or not they will be approved largely depends upon the C.A.B.'s reaction to the conclusion reached by Professor Cherington that there is a prospect of considerable excess capacity in the industry after 1959, which necessitates some new thinking about price policy. The situation outlined in Professor Cherington's report to the President seems to call for a much more radical reappraisal than he suggests in this book.

Answering his second question, Professor Cherington is not convinced that the airlines have adopted a sufficiently scientific approach to price determination. He suggests that " the feel of the market " has been a more general guide than detailed analysis of existing and potential traffic. He also suggests that management concern with pricing has been sporadic and believes that organisational changes are desirable within the airlines to secure a continuous top-level review of price policy. It is interesting to reflect how far these strictures apply to airlines outside the United States. There can be little doubt that it is generally true throughout the international industry that far less top-management attention is given to pricing than to equipment policy.

The part of the book which is likely to be of widest interest is that which deals with the regulatory environment in which airline price policy is made and operated. Air transport is treated in the United States as a public utility and the system of economic regulation embodied in the Federal Aviation Act owes much to earlier railway legislation. Professor Cherington's handling of this part of his subject is descriptively interesting but rather unsatisfying from an analytical point of view. The concepts of " undue or unreasonable preference " and " unjust discrimination " have played a very large part in U.S. transport regulation. Whilst it is interesting to learn how the C.A.B. has interpreted its obligations to prevent such transgressions, one looks in vain for an appreciation of whether these interpretations have made economic sense. It may be that preference and discrimination have so much become the province of lawyers in the United States that economists hardly dare to have an opinion. If so, this is unfortunate, for there is a great deal that the economist can say on this subject. Reduced fares available only in conjunction with an inclusive tour—a widely used tariff device in Europe—provide a good example. The C.A.B. forbade this kind of fare in the United States on the grounds that " such discriminatory practices are a far cry from the equal treatment the public is normally entitled to expect from a public utility ". The economist is entitled to reply that if average costs will fall with increased traffic, that if the fares proposed are not lower than marginal costs, and that if these fares will, without materially diverting existing traffic, create new traffic which would not otherwise travel by air, there is a very strong case for believing that such fares are in the best public interest and justly discriminatory. Without this sort of approach the Board is continuously thrown back to the notion that " unjust discrimination results if different fares are charged for like and contemporaneous services for like traffic under substantially similar cirsumstances and conditions ". But despite this aberration there can be little dispute about Professor Cherington's general conclusion that C.A.B. regulation has had positive beneficial results, even though—or perhaps because—the Board has left the initiative in price policy largely to the airlines.

Airline Price Policy is a book which can be recommended to a much wider readership than air transport practitioners and specialists. It will appeal to all who are interested in how this important aspect of policy making is actually conducted in a regulated industry.

STEPHEN F. WHEATCROFT.

The Economic Development of Radio. By S. G. STURMEY. Gerald Duckworth & Co. 1958. 284 pp. 30s.

The complaint is frequently made that economists have paid too little attention to technological innovation. In particular, theories of competition are usually based on restricted assumptions to the effect that technical possibilities are not only given but fully exploited by managers who always combine the factors of production in what is technically the best way, however far they may fall from grace in other respects. It can be pointed out that conclusions drawn from such narrow assumptions can be only a dubious guide to public policy, and it may also be regretted that so many other issues, on which economists might usefully pronounce, have been often neglected. Such complaints are not unjustified and there is increasing evidence that economists are taking them seriously. International comparisons of productivity, open to criticism though they have been, were a valuable stimulus in the post-war years, and more recently there have been various studies of innovation and the difficulties that beset it, some of them made possible by grants under the Conditional Aid Scheme. The latter include a series of studies by members of the Department of Political Economy at University College, London. Dr. Sturmey's volume belongs to this series.

The radio industry, chosen by Dr. Sturmey for the study of innovation, has received comparatively little attention either from independent economists or from the public committees that have provided so much information about some of the older industries. Dr. Sturmey has therefore been obliged to do a good deal of pioneering work and his task has clearly not been an easy one. If the study of innovation is important, it is also extremely difficult in an industry where the technology is as involved as it is in the radio industry. Dr. Sturmey's labours have been immense; he has worked his way through much of the technical press and presented his findings in the case of three innovations: marine radio, point-to-point radio communications and broadcasting. Unfortunately one gains the impression that the book was completed too quickly. There is an immense amount of detail but the economic implications are not always kept in sight. Moreover, if he was resolved to write at some length about technical change, there was probably a case for inserting somewhere a simple explanation of the main technical issues for the benefit of his scientifically illiterate colleagues.

In the concluding chapter the main questions are clearly set out. There one can read in summary the author's views on such matters as the attitude of the cable companies to radio, the effect of official regulation by the Post Office and the consequence of establishing a public monopoly in broadcasting. There, too, the structure of the industry is described by dividing firms into " branchers ", " assemblers " and " specialisers " who produce " specialisms ". (Why not " specialists " who produce " specialities " ?) These comments on structure are unfortunately too abbreviated and the reader will search in vain throughout the book for an adequate treatment of this somewhat basic matter. The finance of innovation is also mentioned in the concluding chapter but surprisingly neglected elsewhere. On the other hand the reader will discover in this last chapter such statements as these: " The important question for evolutionary innovation is that regarding entry "; " It is not actual competition which produces evolutionary innovations so much as the fear of competition by established firms and the desire to compete by others"; "Revolutionary innovations do not usually arise from within an industry, no matter what its structure". "Atomistic competition in radio did not prove favourable to invention until some of the more enterprising firms saw their chance to avoid atomistic competition by innovations based on their own research"; "Restrictive patent policies, save on absolutely master patents, are good for technical progress and appear to be against the interests of the patentees". What is really interesting is to take such challenging statements as these and then work back through the book in order to test how far they appear to be supported by the mass of evidence Dr. Sturmey has collected about this industry.

Although Dr. Sturmey's book could have been improved by better planning, it represents a courageous attempt to deal with a difficult industry and constitutes an important source of information for students of industrial economics.

THOMAS WILSON.

Decision-making and Productivity. By S. MELMAN. Basil Blackwell. 1958. xii + 260 pp. 27s. 6d.

The principal conclusions emerging from Professor Melman's detailed examination of the post-war experience of the Standard Motor Company of Coventry are that decisions affecting production are made by workpeople as well as by managers; that a process of joint decision-making is possible and desirable because it promotes industrial self-government and facilitates the productivity process; and that the alternative to joint decision is competition for managerial control. Subsidiary conclusions suggest that the marshalling of labour into large groups for the purposes of production and payment is conducive to balanced output by encouraging greater co-operation and closer integration amongst the individuals and groups involved and that large-scale highly-mechanised plants do not require large and ever-expanding managerial groups.

Much of the work is taken up with establishing the not unsuspected fact that there is no managerial monopoly of decision-making in industry. For analytical purposes, many writers have distinguished between decision-making and execution, between thinking and doing, but few people with practical experience in these matters can harbour the illusion that in fact management alone decides. The idea of multiple decision-making agencies is neither as new nor as revolutionary as the author seems to believe. As is indicated in the final chapter, the phenomenon occurs widely in industry; but the author seems to suggest that it is more highly developed in the Standard Company than elsewhere in Britain. Those familiar with British industry will, however, know that in many instances (in iron and steel, printing and the post office for example), workers and their unions possess even greater powers of participation in, and control over, managerial decisions than their counterparts at Standard Motors.

The thesis, developed at great length, that the process of joint decision-making as practised in this firm made a distinctive contribution to productivity and to industrial democracy is not entirely convincing. The facts seem quite capable of explanation in terms of normal industrial experience in which decisions by workers and unions have always played a part. It is also difficult to accept the view that competition for managerial control by way of nationalisation is a policy which may be taken seriously by British trade unions, which have quite clearly rejected participation in the management of nationalised industries in favour of purely consultative arrangements. There is thus no conflict, as envisaged by the author, between nationalisation and industrial democracy. Manipulation of trade union activities by political groups unquestionably exists, especially at the local level, but in general the movement regards itself primarily as the guardian of industrial interests which it has consistently refused to subordinate to political considerations.

As to the subsidiary conclusions, it is debatable whether bonus payments on the output of very large groups have the consequences which are attributed to them. The system as operated at Standard seems to approximate to a high day-wage system which would be quite feasible and would produce much the same results in circumstances in which, as we are informed, 75 per cent. of the work up to 1956, and thereafter about 85 per cent., was primarily machine-paced. It would seem, too, that this factor rather than the alleged self-regulating character of the large group was mainly responsible for the low ratio of supervisory staff to production employees.

N. S. Ross.

The Blackcoated Worker. By David Lockwood. George Allen & Unwin Ltd. 1958. 224 pp. 21s.

Within a generation or so the salariat may well constitute a majority of exployees. Taken with the persistent growth of non-manual unionism since the First World War, this means that black- (or white-) coats may also then be, if not a majority of trade unionists, at least a group exceeding in influence, say, the organised engineering operatives. Traditionally the white-collar worker's outlook has differed so radically from that of the manual wage-earner as not merely to divide the employed massif by a social gulf, but to induce very different policies of collective organisation—distinctively on such things as strike action and political association. But on the other hand, the non-manual employee has also been submitted in the last generation to an egalitarian economic process which, although part of a general levelling-out of wage and income differentials, might be construed in his case as one of "proletarianisation". What, then, will be the labour movement's character in twenty years' time ?

Mr. Lockwood's study of the clerical worker is a most interesting contribution to the discussion of this problem. Its central pre-occupation is the clerk's "false class consciousness". "Among no other class of the population", he says, "has the image of status operated so powerfully to influence the behaviour of its members." This exaggerated status-consciousness he attributes to the clerk's "social marginality" between the wage-earner and the middle-class, which breeds a vicious circle, of snobbishness on his part and contempt on the manual worker's, that forms a "self-perpetuating status barrier". Thus the clerk's social/economic declension has by no means automatically entailed his acceptance of (or into) a working-class standing. In fact Mr. Lockwood suggests that the dissimilar histories of the major clerical trade unions show that in this testing respect—of the clerk's willingness to organise and of his unions' willingness to link with the manual labour movement—different groups of clerks have displayed very different reactions.

This argument is admirably done, with a gift for orderly analysis and classification, and keen insight. Mr. Lockwood is perhaps at his best in revealing the quality of the clerk's sense of social status, and its reflection in his associations' internal controversies. But his thesis seems limited by a sociological emphasis which rather weakens its comparative basis. In particular, the opposing "image" of the organised manual worker which it implies seems unrealistic. For instance, Mr. Lockwood explains the late growth of clerical unionism by the historically scattered, personal and promotable employment of clerks in the " counting house "—and his chapter on this is quite excellent—by contrast with the wage-earners' massing together under impersonal factory managements, which he supposes inevitably induced manual unionism. But widespread organisation of factory workers hardly antedates that of clerks. Apart from one or two groups favoured by special circumstances, the unions' earlier strength lay amongst workers, craftsmen or others, who were also mainly employed in small groups, were promotable, and were economically and socially separated from the generality of wage-earners. The key to their solidarity was a tradition of collective identity and common standards; so that the key to the clerks' one-time *lack* of solidarity was an opposite tradition of personal identification with the employer.

Again, and logically, from his starting position, Mr. Lockwood apparently explains the recent growth of clerks' unionism largely by the "bureaucratisation" of major clerical employments, which introduced to them large-scale organisations with impersonal rules and work-relationships. That this explanation just does not fit is apparent from his own discussion of N.A.L.G.O.'s success—which itself "bureaucratised" local government. The argument also neglects the weakness of some manual workers' organisations. (One third of wage-earners are still under legal wage regulation.) These unions failed to pass the critical stage for modern manual unionism's growth, the achievement of recognition by employers. And one would expect (even if experience did not suggest it) that such recognition is still more critical to the organisation of non-manual employees. The clerical unions that have become strong are mostly those which, like the biggest wage-earners' associations, were thrust into a recognised status at about the second decade of this century by the general (and near-political pressure on employers). Mr. Lockwood's inadequate image of the manual worker shows itself, too, in his discussion of the clerk's changing economic status. The recent "levelling up" of wages to salaries is, as he shows, less a matter of comparable "rates" than of average "earnings". And for wage-earners these reflect particularly the persistent decline in the proportion of unskilled (and therefore lower-paid). On the other hand, Mr. Lockwood gives rather little attention to the actual work of clerks, and notes the immense increase in the proportion of women (historically, a major source of less-skilled labour) mainly as an aspect of the lowering of the "masculinity" and hence the social status of clerking. One wonders how complete the modern clerk's image of himself may by. Set his main occupational requirement, the now universal skill of literacy, against the constantly rising skill demanded of manual labour, and may it not be that the manual worker is really the "superior" being ?

H. A. TURNER.

Colonial Planning : A Comparative Study. By BARBU NICULESCU. George Allen & Unwin Ltd. 1958. 208 pp. 18s.

It requires but a cursory knowledge of the British Colonies to know that the *Quarterly Journal of Economics*, studded with economists' investment criteria, finds no place on development committees' tables (perhaps rightly so !). However, how such countries do allocate their scarce capital and skilled manpower between different sectors has remained something of a mystery outside narrow official circles. The function of Mr. Niculescu's study is to show us, as far as possible, the decision-making machinery at work. He concentrates on the British Colonies, with lesser reference also to territories under French, Belgian and Dutch control.

Mr. Niculescu's detailed account of the machinery of planning covers types of decision-making committees, budgeting arrangements, plan-revision procedures, and so on. The picture is one of enormous diversity, and ever-shifting arrangements even in single territories. These switches in policy, between, for example, " social " and " economic " expenditure, between big and small-scale projects, are shown to emanate both from metropolitan politics and local events. Mr. Niculescu has obviously done an immense amount of work on the heterogeneous collection of colonial reports available.

This material, forming the bulk of the book (Parts III, IV and V), is not a study in economics so much as in politics and administration; I have already indicated its value. The economic aspects, at the beginning and end of the book, leave less cause for satisfaction.

Two questions may be raised: on the scope of the economics included and on its detailed content. The scope includes short discussions on such points as the idea of balanced growth, sterling assets and capital aid, emulative consumption and low savings in poor countries, etc.; all these are points much more extensively discussed elsewhere. Strangely enough, the field of theoretical economic work which appears particularly apposite to the administrative discussion, namely that of investment criteria, is not mentioned. This might preferably have determined the scope of the economic sections.

Whilst Niculescu is strong on administrative detail, some of the statements in his economic discussion cause one to stop and ponder. We read at the outset that " it is to be hoped, maybe even expected, that standards of output and consumption will in time be equated throughout the world ". The value judgment apart, why this is to be expected is not explained. It would not appear to be the logic of economic processes; new and massive political intervention would seem necessary to bring it about. That equalisation is the ultimate normalcy underlies other propositions. Education expenditure in developed countries is said to be high-cost and low-yield, while the opposite is true in low-income countries (p. 182). But might not the same amount spent here on education in the use of computers, as in Africa on literacy, bring a higher return here? Marginal rates of return to such expenditures vary with technology, itself ever-changing.

One finds oneself questioning other statements. The ill-advisability of using complex machinery in primitive agriculture is put down to the need for the carrying of costly stocks of spare parts (p. 33); no mention is made of the more fundamental aspect that the productive method in question is capital-intensive. Again, on the subject of skilled labour, the cost to the colonies is " disproportionately " high due to their having to enter the international labour market (p. 34); might not the " marginal product " also be " disproportionately " high?

Blemishes can indeed be found in the economic arguments of this book, but those sectors on administrative procedures are a solid achievement, and deal with a need in the literature of development.

DOUGLAS DOSSER.

The Economic Growth of Hong Kong. By E. SZCZEPANIK. Royal Institute of International Affairs. Oxford University Press. 1958. xiv + 186 pp. 25s.

For a hundred years Hong Kong lived and grew by entrepot trade, by selling other countries' products. Since World War II the Colony has relied more and more on selling its own manufactures, which are now 25–30 per cent. of its total exports.

In common with Professor Benham, I have long urged that those interested in industrial growth might learn more from studying this experience than from all the fashionable discussion of " balanced growth ", " industrial take-off ", " capital/output ratios ", etc. Mr. Szczepanik's book is a valuable contribution to that study. He describes both the broad features and the details of Hong Kong's " industrial revolution " and traces its origins in the combination of immigrant Chinese labour, capital and (above all) enterprise with a British government of almost nineteenth century outlook—perhaps the best working model of laissez faire capitalism still extant. A mass of statistics illustrates the story. But, rather oddly, there is no table of comparative totals of domestic exports among the 46 tables of his statistical appendix. And he slips up occasionally; Hong Kong's earnings and profits taxes, with taxes on other kinds of income, date back to 1940, not 1947, and a part of the sterling backing for the currency is still held by the Banks rather than the Government Exchange Fund.

There remain, however, many fascinating aspects of this phenomenon not covered by Mr. Szczepanik. For example:

(1) Was the impetus to industrial growth already fully established before 1939 ? Some people both in the Colony and in the Colonial Office thought so even then; Hong Kong's exports of rubber shoes worried London nearly as much in the thirties as textile exports do now; industry was already moving in from Shanghai after 1937; and under war stimulus manufacturing grew rapidly in 1940–41.

(2) Did Hong Kong gain any net advantage from the unexpected end of the war in 1945 ?

(3) What part have the banks played in financing industry, expecially just after the war ?

(4) Has total entrepot trade (as distinct from trade with China) actually declined or is the industrial growth simply an addition to a maintained commercial activity ? And how important have the commercial contacts been in facilitating the industrial growth ?

SYDNEY CAINE.

The Design of Development. By J. TINBERGEN. Johns Hopkins University Press, Baltimore. Oxford University Press. viii + 99 pp. 20s.

This short book is the second publication of the International Bank's Economic Development Institute. Its object is to set out in simple and concise language the purpose of development planning and some of the major economic considerations to be borne in mind in evolving a policy of economic development. It succeeds admirably. Professor Tinbergen outlines the major characteristics of economic programming. However crude or complex the techniques used, the basic objective is to ensure consistency between different parts of the programme and between the programme and resources. He draws his readers' attention to some of the more advanced methods of programming. He advocates their use wherever possible; but he stresses the over-riding importance of common sense and following one's nose. He has much good advice to offer on the respective roles of public and private activity, the scope for protection, inflation and a number of other issues.

At the same time, one wonders whether he is not too optimistic on some points. He is clearly a believer in the capital coefficient, and undoubtedly it has its uses. But we are still highly uncertain about its magnitude, especially in underdeveloped countries. Moreover, we cannot argue, as Professor Tinbergen does on pp. 14–15, that the coefficient shows the importance of checks on population increase. That is a legitimate conclusion for some underdeveloped countries but by no means for all. The effect of population increase on the volume of capital formation required for any given rate of economic growth is only one of the considerations to be kept in mind in deciding on this question.

Professor Tinbergen is particularly concerned to urge that technologies employed should be adapted to the factor availabilities of the country. A common characteristic of underdeveloped countries is an abundance of labour relative to capital; and he stresses the importance in such a situation of using labour-intensive methods. He emphasizes, moreover, that where there is unemployment " accounting prices " (i.e., opportunity cost) are more relevant than market prices. He joins in the criticism of technicians who employ capital-intensive " advanced technologies " in countries where capital is scarce and labour plentiful. All of this is very much to the point; but it is a pity that the brevity of the book has apparently precluded consideration of some possible qualifications. The relative proportions in which capital and labour are available in underdeveloped countries predispose us to the conclusion that capital-intensive methods will exhaust the capital supply at a lower level of output than that at which labour-intensive methods reach the labour limit. But this is by no means an inevitable conclusion. The production function may be such that labour-intensive methods, given the available labour supply, yield a lower output. Thus the technician who selects the " advanced technologies ", in apparent disregard of the relative proportions in which factors are available, may after all be following the best course to maximise output. We simply do not know sufficient about the production function in relation to capital and labour supplies to be as categorical on this point as we often try to be.

A. D. KNOX.

Studies in Economic Development, with Special Reference to Conditions in the Underdeveloped Areas of Western Asia and India. By ALFRED BONNÉ. Routledge & Kegan Paul. 1957. 294 pp. 32s.

The chief interest and merits of Professor Bonné's book lie in its numerous international comparisons: comparisons not only between a number of under-developed countries, but also (in various respects) between advanced and under-developed areas. These comparisons help to keep fresh in the reader's mind both the large gap between different types of economies, and also the great differences between under-developed countries themselves. On the other hand this method has palpable dangers—not always avoided by

Professor Bonné—because owing to variations in definitions and in the compilation of statistical data, and also to the great unreliability of such data in many under-developed countries, false conclusions can easily be drawn in the absence of full explanations of the figures quoted. To provide such explanations would require a very detailed knowledge of the methods of collection and weaknesses of each country's economic statistics, and would, in any case, have added very substantially in the length of the book (which would, in its turn, have seriously interrupted the general flow of the text).

Unfortunately the value and interest of a considerable number of the tables (for instance in Chapter VII on " Industrialisation in Under-developed Countries ") are marred by their not being up-to-date, even when they refer only to one country.

What Professor Bonné has set out to do is, first, to describe economic conditions and problems in his chosen range of under-developed countries, and to point the contrast with advanced countries. Next he lists a number of important factors accounting for the high level of net output in developed countries, and shows how the absence, or limited extent, of these factors in under-developed countries has affected their productivity. Thereafter he discusses the means by which changes leading to economic adjustment could be introduced. After a discussion on the history of theories of economic growth he ends with an optimistic appraisal of how " implanted economic development ", in the form of foreign assistance, Governmental leadership, and the adoption of Western techniques, could replace, in particular, the lack of domestic resources and potential savings, which he holds to be quite inadequate to inaugurate progressive development.

There is nothing very novel in this approach, but Professor Bonné writes persuasively and brings great experience, knowledge and common sense to the support of his analyses and arguments.

VERA ANSTEY.

The Income of Nations. By P. STUDENSKI. New York University Press. 1958. xxii + 554 pp. $25.

The number of pages and the price are sufficient to indicate that this is a book of elephantine proportions which not many are likely to read through from beginning to end and still fewer are likely to buy.

Despite the forbidding appearance of this tome (for once, this noun really is appropriate), there is some most interesting material in it. The author ranges very widely over the whole field of the history, logic and techniques of national income estimates. By far the most interesting section is the historical part which not only covers the usual ground of William Petty and Gregory King and their British successors but also deals in some detail with the French and Russian contributions. The description of how National Income studies started in Russia in the late eighteenth century but were then proscribed as subversive activities (one author being exiled to Siberia for his sins) until the early years of the twentieth century is completely fascinating.

Part II deals with theory and methodology and although competent and indeed fairly comprehensive, is somewhat pedestrian. There are some interesting instances of various attempts to value housewives' services and consumer durables; but one wishes that rather more attention had been given to modern developments such as the money flows approach of the Federal Reserve Board in Washington, or the very considerable amount of work emanating from the Department of Applied Economics, Cambridge, on the calculation of national real product. Attention to the latter might have saved the author from the error (pp. 244 ff.) of omitting changes in the terms of trade from the list of factors which can cause changes in real national income in a country.

Part III describes in some detail the history and methods of national income estimation in thirteen selected countries. No two people would agree on their selection of countries for detailed treatment, but it is regrettable that the author did not pay more attention to some of the particular difficulties of concept and estimation in the most under-developed parts of the world. Although he examines India, Brazil and the West Indies, he does not really get to grips with the fundamental difficulties of compiling (or using) national income estimates in such countries.

Part IV discusses developments in the field in 66 other countries, albeit in less detail. It is here that the inability of any one man to be an encyclopædia leads to some trouble. There are some obvious gaps in references to U.K. estimates and to work in British colonial territories. Maybe the same is not true of the other countries dealt with, but one is inevitably left with some doubts.

No reviewer could possibly advise anyone to read this book from cover to cover. But as a comprehensive work of reference and as a source of historical information it will certainly have its place.

A. R. PREST.

Thorstein Veblen: A Critical Reappraisal. Edited by D. F. DOWD. Cornell University Press, Ithaca. Oxford University Press. 1958. pp. xii + 328. 40s.

Despite the numerous commentaries on Veblen's work that have appeared in recent years, and the fact that he is still occasionally referred to as America's greatest social theorist, the centenary of his birth received comparatively little notice. This volume embodies the results of the two principal commemorative occasions, both organised by economists: three papers presented to the American Economic Association (by J. Dorfman, A. Gruchy and P. Sweezy) and eight lectures delivered (by the late W. Hamilton, C. Ayres, N. Kaplan, F. Hill, L. Fishman, P. Morrison, C. Goodrich and D. Dowd), at Cornell University, one of the institutions of higher learning at which Veblen sojourned briefly. Six further essays (by M. Copeland, L. Nabers, M. Brockie, J. Dirlam, M. Watkins and G. Zinke) complete the volume.

The unevenness of these essays is no surprise, when the divergent ages, backgrounds and interests of the various authors are taken into account. There are substantial differences, and several sharp conflicts of interpretation, and no doubt more could be readily discovered if the reader were provided with an index in lieu of the reprint of Dorfman's bibliography of Veblen's works. But while the general tone is sympathetic to Veblen, and his insights and his influence are sometimes overrated, the volume contains few of the eulogistic excesses that so often accompany publications of this kind. In this respect the collection reflects the current state of American critical opinion on Veblen, which has almost reached an equilibrium between the adulation of a small band of disciples from the early 1930's to the middle

1940's, and the effective though unnecessarily destructive critiques of the ensuing decade. The contributors, most of whom are new to this field, are less concerned with praise or condemnation than with the attempt to consider the relevance of Veblen's ideas to present-day research and policy problems.

This collection does not fulfil the main need of Veblen scholarship, namely, a thorough and impartial study of his influence on American economic and social thought; but it suggests that such a study would be eminently worth while. The most valuable essays are those concerned with such neglected phases of Veblen's thought as idle curiosity, business cycle theory, corporation finance, American labour, and the ideology of the engineers, or with such current preoccupations as underdeveloped countries and technological change. There are balanced studies of his instinct theory and of the perplexing question of his relationship to Marx; but the essays reviving old controversies merely repeat old misconceptions, and those discussing his theory of growth, his view of American capitalism, and his theory of cultural evolution are unduly favourable. On the whole, however, the book will help English economists to understand the continued American interest in a thinker who is unduly neglected in this country.

A. W. Coats.

Black Dyke Mills. By Eric M. Sigsworth. Liverpool University Press. 1958. xi + 385 pp. 35s.

The Rise of the British Rubber Industry During the Nineteenth Century. By William Woodruff. Liverpool University Press. 1958. ix + 246 pp. 35s.

Enthusiasm rather than understanding has for long been the chief characteristic of most writers of British business history. A higher proportion of competent studies is, however, beginning to raise the general standard and these two volumes, both of which have grown out of doctoral theses, are notable additions. Dr. Sigsworth has dealt with the Queensbury worsted firm of John Foster and Son, and Dr. Woodruff with the rubber concern which Stephen Moulton established at Bradford-on-Avon. Both books are confined to the nineteenth century and both aim to describe the growth of the branch of industry in which the business operated as well as the development of the firms themselves. Dr. Sigsworth, making skilful use of a wide range of West Riding sources and of local knowledge, has devoted a third of his book to the industrial background: the shift of manufacture from East Anglia, the transition from domestic to factory production and the changes in fashion and marketing. The general picture is clearly in the reader's mind before he is introduced to the Fosters' particular problems. Dr. Woodruff has not been so successful in his treatment of the industrial background, but this is partly because his sources were more difficult to handle: patents and extravagant claims of patentees are among the most confusing of documents. Moreover, he has only dealt with the background in a connected way prior to Moulton's appearance on the scene in 1848. Although the records of the far larger Macintosh and North British companies have been consulted, they do not appear to have been very helpful. The viewpoint from 1848 onwards, therefore, is very much that of the Moulton firm, a highly specialised undertaking from which it is very difficult to build up a picture of the rise of the rubber industry as a whole.

From both these studies the dominating role of the business man emerges most strikingly. The Fosters and the Moultons succeeded because they were quick to grasp an advantage. Fosters made the most of the expanding market which resulted from the introduction of mixed worsted fabrics at the end of the 1830's and was to develop quickly as population and real wages grew. At the same time, they were supplying continental weavers with yarn in great quantity. Their correspondence with a relative who worked in a large weaving factory in Silesia and acted as their confidential informant provides the most entertaining and, in many ways, the most revealing part of the book. Stephen Moulton, who associated with pioneers of the rubber industry in America during his residence there—a very shadowy part of Dr. Woodruff's study—received substantial American aid when he set himself up as a rubber manufacturer in England. After a faltering start, he made money out of the Crimean War by supplying waterproofs and then developed the manufacture of rubber mechanicals, particularly for railways. Both books contain a wealth of detail—in Dr. Woodruff's case too often hidden in the footnotes—about the problems which confronted these nineteenth century business men.

T. C. BARKER.

The Eastland Company and the Common Weal in the Seventeenth Century. By R. W. K. HINTON. Cambridge University Press. 1959. xi + 244 pp. 32s. 6d.

The subject of this book, says Dr. Hinton, is " the formulation of policy, its execution and results, in regard to the Eastland trade ". In pursuing this study he takes the familiar view that the role of the regulated companies as agencies for state control was taken over in the course of the seventeenth century by the centralised administration of the state, which thus destroyed the justification for their privileges. Examining this in detail, in the light of the relations of the Eastland Company with governments, he concludes that the decisive change came sharply with the Navigation Act of 1651, when the Republican Government resumed some of the delegated powers: from that time the process of bureaucratic centralisation went on uninterruptedly. The discussion is clear-headed and sensible, and throws some much-needed new light on the ever disputed background of the Navigation Act of 1651: it would have been even more valuable if the special experience of the Eastland Company had been compared with that of other organisations—in particular the Merchant Adventurers and the Russia Company—which shared, in one way or another, its decline in status.

This is well trodden ground; and many readers will find more interest in the new material (specially good for the first half of the century) on the development of Baltic trade. Briefly, there are three elements in the changes Dr. Hinton describes. First, the cloth trade was declining, almost from the beginning of the century, under the impact of Dutch and Silesian competition. (One might suspect, however, that this decline was partly attributable to a renewal of indirect trade through Hamburg and Middleburg, which had many advantages over the long sea haul for the carriage of valuable and easily damaged goods.) Then corn imports, which had been the counterpart of cloth exports in the sixteenth century, disappeared completely; while lastly, in their place, there was a steady and rapid growth in the import of " naval stores "—timber, pitch and tar, hemp and flax, iron—which turned English interests from Danzig, Elbing and Königsberg to Gothenberg, Stockholm and Riga. Long before mid-century the rising tide of imports had exceeded in value the declining cloth export, and England developed a growing adverse balance of trade with the Baltic, which caused some alarm late in the century.

The writer makes it clear that the particular function of the Eastland Company had always been the control of the practices of cloth export to Danzig and Elbing; it had never concerned itself much with the importing activities of its members. We might draw a conclusion different from Dr. Hinton's, therefore, that as the subjects of the Company's interest, and the places where its attention was concentrated, became less and less representative of the main business of its members, and of England in the Baltic, its decline was inevitable whatever the actions of government. Dr. Hinton would have further assisted our understanding of these changes if he had given some space to the practical details of the carrying on of trade by English merchants at home and abroad, and to the general background of the economic development of the Baltic area. Despite these limitations, however, his work is a most useful contribution to English commercial history.

RALPH DAVIS.

SHORTER NOTICES

Economic and Philosophic Manuscripts of 1844. By Karl MARX. Lawrence & Wishart. 1959. 209 pp. 3s. 6d.

This book gives us in English translation Marx's first essays in political economy, in the form of three rough fragmentary drafts dealing with Wages, Capital, Profit, Accumulation, Rent, and Private Property and Communism. Engels's essay " Outlines of a Critique of Political Economy " (1844) is included as an appendix. Most of the main Marxian themes are to be found in embryonic form, and there are long quotations, of much interest to the student of sources, from such obscure contemporaries as Buret, Pequeur. Loudun and, in particular, Wilhelm Schulz, author of *Die Bewegung der Produktion, eine geschichtlich-statistische Abhandlung* (1843). Marx appears as an extremely angry young man, claiming, of course, that his " results have been won by means of a wholely empirical analysis ". Whether or not there was more to be angry about in 1843 than there is now, he was at any rate able to rationalise and work out his anger in constructing his gigantic " scientific"-prophetic system, surely almost an intellectual impossibility as an outlet for an intelligent A.Y.M. today.

The Journal of Industrial Relations. Volume 1, No. 1, April 1959. (Edited by Kingsley Laffer at the Department of Economics, University of Sydney, for the Australian Industrial Relations Society. 10s. per copy. To appear twice a year.)

Australian industrial relations have special features that have long attracted the attention of students throughout the free world. There has grown up also in Australia a vigorous body of experts in the various issues of industrial relations as those appear not in Australia alone. On both scores a warm welcome will be extended to the journal which now appears, and which promises to form part of the international literature of the subject.

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